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HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES.
JANUARY—JUNE,
1859.

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THE
HALF-YEARLY ABSTRACT
OF THE
MEDICAL SCIENCES:

BEING
A PRACTICAL AND ANALYTICAL DIGEST OF THE CONTENTS OF THE PRINCIPAL
BRITISH AND CONTINENTAL MEDICAL WORKS PUBLISHED
IN THE PRECEDING SIX MONTHS:

TOGETHER WITH A
SERIES OF CRITICAL REPORTS ON THE PROGRESS OF MEDICINE AND
THE COLLATERAL SCIENCES DURING THE SAME PERIOD.

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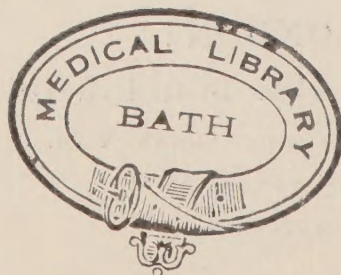
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HALF-YEARLY ABSTRACT

OF

THE MEDICAL SCIENCES,

&c. &c.

PART I.

PRACTICAL MEDICINE, PATHOLOGY, & THERAPEUTICS.

SECT. I.—GENERAL QUESTIONS IN MEDICINE.

(A) HYGIENE.

ART. 1.—*The Meteorology of 1858.* By Mr. HINGISTON, of Brighton.

(*British Med. Journal*, March 19, 1859.)

THE year 1858 was a remarkable one, both in a meteorological and sanitary point of view. There was an eclipse of the sun and moon in the spring, and a brilliant comet in the autumn; the mortality of the year was above, and the marriages and births were below the average. Indeed, during the last two weeks of November, the deaths were in excess of the births; and the natural increase of the population, which has been constantly progressive for so many years past, was, during this short interval, suddenly reduced below its steady and customary ratio. The natural increase of the population for the United Kingdom in 1858 was not more than six hundred and forty-two daily, ('Quarterly Return,' 40, December 1858, p. 2; whereas it had been throughout the preceding year 1857 as high as one thousand and twenty-one a day ('Quarterly Report,' 38, July 1858, p. 2. The last three months of the year 1858 were particularly unhealthy; and the Registrar-General declares ('Quarterly Return,' 40, p. 1, 'that it has never before happened that so many deaths were registered in any autumn quarter, as in this last.' The mean mortality for 1857 was less than twenty-two in a thousand ('Annual Summary' of 1857',) p. 4, but the mean mortality of 1858 was twenty-three in a thousand ('Quarterly Return,' 40, December 1858, p. 5. The year 1858 may, therefore, be regarded as a very unhealthy one.

The year began at the usual temperature of the month of January, the first and third weeks of which were as much as from three to four degrees below the average of the season. February was cold nearly throughout. Snow fell on the 2d, and lay upon the ground; and it continued to fall occasionally till the end of the month. On the night of the 26th, there was a partial eclipse of the moon, and the thermometer sank as low as 23° Fah.; the frost was severe, and the ice two inches thick. March was still colder than February, and the temperature of the first fortnight was as much as 8° Fah. below the average. Snow fell, and lay upon the ground, from the 1st to the 6th of the month. The frost then gave way to a partial thaw, with hail, rain, and sleet, followed by a heavy snow-storm; and the ground was again white from the 11th to the 13th. This hard weather again ended in hail, rain, and sleet, and finally in a complete thaw. On the 15th, there was an almost total eclipse of the sun at 1.10 o'clock, p.m. Previous to the eclipse, the electricity had been positive, and the weather cold, but subsequently it became negative, and the air warm, the temperature averaging about 3° in excess. The mean reading of the barometer throughout the first three months of the year was nearly half an inch in excess; and for the first fortnight after the eclipse, it stood above thirty inches, and, on the 22d, rose as high as 30.422 inches. April and May were both of them cold, perhaps about 1° lower than the average; but June was hot throughout, and was said to be hotter than any month of June since 1771, except the June of 1846. The barometer stood unusually high, being scarcely less than thirty inches all the month. The weather was bright and pleasant. July was cold, and it was in the middle of the month about 5° in defect. August was fine, and September warm, and at one time 4° in excess. The air was remarkably dry, clear, and luminous. The harvest began early. Wheat was cut on July 17th, or nearly three weeks earlier than usual, although the yield of corn was not so very great. There were severe thunder-storms in July and August; and the aurora was seen at Liverpool on July the 16th.

A singularly brilliant comet appeared in the N. W. in September and October.

October was fine. It began with the aurora, and strong gales. A few cold fresh days excepted, it was nearly 2° above the average. November, on the contrary, was extremely cold; the mean temperature of the 23d and 24th was reported at Greenwich to have been lower than any two consecutive days in November during the last forty-five years. On the 24th, the thermometer sank 12° below the freezing point of water. The mean temperature of the month was not more than 39.6° Fah. In Paris the thermometer was 10° lower than in England, and in Austria it was reported 10° lower than that. There were heavy falls of snow in the north of Great Britain, and also in Belgium and France. At Lyons the streets were nearly blocked up with it. The same weather occurred in Italy. In England, thick mists prevailed throughout the quarter, and November was particularly remarkable for several very dark fogs. On the 15th of this month there was a severe storm of wind, that did great damage to life and property, both by sea and land; and, then, the weather suddenly became warmer, and was nearly 6° in excess of the season. December was likewise warm; there was thunder on the 18th;

and the year closed at a temperature of 1° warmer than usual. There was a great deal of illness everywhere; and the death-rate was higher than in any autumn quarter upon record. It is very remarkable, that, during this sickly period, from November 14th to December the 31st, little or no terrestrial electricity was exhibited by the electrometer, and that what little showed itself was, with the exception of December the 22d, uniformly weak and negative. At the same time, the humidity of the air was almost "plus," as it was denoted by the figures 81 or 92, complete saturation being 100. No state of atmosphere could be more adverse to health and life than this.

The wind blew from the S.W. for about two hundred days; all the rest of the year it was from the N.E., with some few exceptions from the N.W., and still fewer from the S.E. The mean horizontal force of the wind was about five hundred and fifteen miles a week, according to Whewell's anemometer, Greenwich; which is considerably stronger than in the preceding year, 1857, which averaged scarcely more than two hundred miles a week. The first week in December was the calmest of the year; and the third week in December, and the second in May, the most blustering. The vernal equinox was calm; the autumnal, tempestuous.

The amount of rainfall in the twelvemonth did not exceed eighteen inches, the annual amount being about twenty-four or twenty-seven inches. It was a dry summer; and in many parts of the country the wells were exhausted, and the pasturage burnt up. It was a good season for wall-fruit, and the orchards were very productive. "I am old enough to remember the comet of 1811, and can distinctly recollect the parched state of the ground in the autumn of that year. This summer I was in the south of France, and can bear testimony to the universal satisfaction expressed at the excellence and abundance of what is called a 'comet-vintage.'

"While travelling I noted the weather; and, in comparing notes on my return, I found that the wet and the fine days on the Continent corresponded almost exactly with those in England. Thus, the 24th of July, which was wet in London, was the same in Paris. The 28th was wet at Poitiers, and also in London. On Sunday, August 5th—a very hot day at Angoulême—the thermometer stood at 100° Fah. in the sun; at Greenwich, and at Bordeaux, it was 80° in the shade. Between August 8th and 16th, I was at St. Nicolas, near Sarlat, in the Dordogne, while the weather, which was so hot here, was the same there; and on the 14th, as I was looking at a sketch just drawn from nature, 'a great and unusual darkness,' to quote the very words of the Greenwich reporter in the Registrar-General's Weekly Returns, under that date, p. 264, 'prevailed about 4 p.m.; the wind changed from N. to W.; rain fell heavily; lightning was seen; and thunder frequently heard.' On the 18th of August, it was rainy at London, as well as at Bayonne; and, while we were exposed to the 'mistral,' that 'scourge of the south,' at Nîmes, a strong gale was blowing from the E. at Greenwich at the same time, viz., September 23rd. The apophthegm, 'Cælum, non animum, mutant qui trans mare currunt' (Hor., lib. i, epis. xi, 27), is, as far as my experience goes, just the reverse of truth.

"The inner life of the earth affects the health, happiness, and prosperity

of mankind, as intimately as the external agencies of light, heat, and electricity, as well as the more dazzling meteoric phenomena, such as lightning, solar or lunar eclipses, volcanic fires, mephitic emanations, and those enigmas of astronomy, comets and candescent aërolites, are supposed to do. Their noxious influence on both mind and body is ancient and proverbial. The past year is a case in point. They are electro-galvanic operations, increasing or intercepting the solar beam, the degrees of heat, and the several combinations of the air we breathe, on so extensive a scale that they cannot fail in modifying the highly sensitive nervous structure of the human frame, whose delicate machinery is set in motion by a current of chemico-vital dynamics, that {excite, suspend, or abolish animal existence, at a touch as fine and decisive as it is permanent and gigantic.”

ART. 2.—*An instance of the Success of Sanitary Measures.* By Dr. FARRE.

(*Lancet*, Dec. 18, 1858.)

The operations of the Board of Health at Macclesfield have been attended with as great success and as great saving of life as at Ely. Their sixth yearly report states, that before the operations of the Board the rate of mortality in the borough was thirty-three in a thousand, while for the last five years it has been twenty-six in a thousand, so that 1015 lives have been saved. In funeral expenses alone, calculated from the returns of 232 burial clubs, £8729 have been saved. But a larger item would accrue under the head of diminished sickness, there having been 28,420 less cases of illness; and the cost of these cases being estimated according to the data furnished by friendly societies, at one shilling a day for twenty days, £28,420 would thus be saved. Here nothing is assumed. Again, an actual instead of an assumed contrast can be presented, with no less agreeable result in the average length of life. The average age of all who died in the first period was twenty-four years (in the adjoining rural district it was thirty-four years); in the last five years it has been twenty-seven years. Each year gains an accession, the last year's average being twenty-eight years and a half. Length of days by three years has thus been added to each inhabitant.

A few years ago, statements such as these received but little favour; indeed, many people affected to ridicule them. Now, however, such vital statistics have assumed an authority which prevents even the ignorant from questioning their real value and tendency.

All the facts are alike conclusive, and point to the same result, and any one will be as intelligible as the rest: the deaths of children under one year have decreased 16·3 per cent., and those under five years 4·6 per cent. These facts are eloquent in argument; and henceforth, if any man would prove that sanitary amelioration is fertile in health and life, and repays with interest the immediate outlay involved, he may refer to Macclesfield as a town whose history offers striking illustrations of that truth.

ART. 3.—*Life and Labour.* By Dr. RICHARDSON.

(Sanitary Review, Jan., 1859.)

In this article Dr. Richardson considers the influence of various common occupations on health and life. The effects of sand-paper making are illustrated as giving rise in the young to a modified phthisis, which is sometimes rapidly fatal. Walking-stick making and hemp and flax dressing are described as exciting bronchitis and bronchorrhœa. The Neapolitan hemp has also the peculiar property of producing a spasmodic paroxysmal attack like that produced by drying hay and by ipecacuan. Trimming manufacturers' work leads to bronchial mischief. Fur dyeing, by exposing the artisans to the fumes of nitrous acid and to the dust arising from dried sulphate of copper, specially leads to a number of serious evils, affecting the teeth, the digestion, and worst of all, the organs of respiration. Cigar and snuff-making, in the first instance, give rise to the peculiar toxic effects of tobacco, and afterwards to chest-disorder, ending in chronic bronchial flux and inanition. The writer of this paper suggests that a Parliamentary committee of inquiry should be organized to inquire into the subject of occupations and health. He concludes as follows:

"When we look at diseases as a whole, we stand amazed at the varieties of type which they assume. When we classify them into groups according to their causes, we stand equally amazed at finding to how very few groups all diseases may be reduced. We find all the disease-causes out of the body and clothed in externals. Resolving the causes, there stand out some dozen poisons of communicable and reproductive power, improper dietary, variations of atmosphere, and occupations. Of all these, the last stands most invitingly for inquiry. The inquiry is of a kind to which the most rigid rules are applicable. Its results might be demonstrations, its suggested remedies simple certainties."

ART. 4.—*On the health of our Army and Navy.*

By Mr. T. SPENCER WELLS, Surgeon to the Samaritan Hospital, &c.

(Medical Times and Gazette, Jan. 22, 1859.)

"Need we remind our readers," asks Mr. Wells, in an admirable leading article in the journal of which he is the accomplished editor, "of the facts told by the Army Commissioners of the extraordinary mortality of our troops? Men picked and selected, by reason of their bodily vigour, dying in numbers far beyond the mortality of any other classes in the country, and of causes evidently in great part of a preventible kind.—70 per 10,000 of the London Fire Brigade die per annum, but of the Foot Guards 204 per 10,000. Of towns Manchester has, perhaps, the blackest lists of mortality to show; its deaths are 124 per 10,000. In all England the mortality is 92 per 10,000; but in the Infantry of the Line it is 187; in the Foot Guards, 204; in the Household Cavalry, 110; and in the Dragoon Guards, 133 per 10,000.

"The Commissioners who investigated the causes of this superabounding mortality, attributed it mainly to overcrowding and want of ventilation

in barracks; but we do not think that they paid sufficient attention to some of the many other influences which, though not immediate, can scarcely fail to be strongly predisposing causes of pulmonary disease. And the misfortune which arises from their not having been duly recognised is this—that the morbid agencies referred to are apt to be too slightly regarded as predisposing causes of disease. It is certain that all diseases, no matter their nature, which produce a cachectic state of the body are strongly predisposing causes of disease—secondary syphilis, for instance—and yet on syphilis the ‘Report of the Army Sanitary Commissioners’ is silent; in fact, the whole matter is there summed up in half-a-dozen lines. Neither does a perusal of the report satisfy us that drinking has not a seriously injurious influence on the soldier’s bodily condition. The Commissioners may have hit on the main causes which blow these pulmonary diseases immediately into life; but they assuredly have not disproved the probability of numerous other agencies coinciding in action, though more remotely and less directly. Besides this, we do not want to know merely what are the causes which occasion the enormous excess of mortality in the Guards beyond what is the ordinary average of the kingdom; we ought to go further, and endeavour to restrain all promoting causes of diseases. It is not enough for us to be told that these other causes do not operate upon the soldier more injuriously than upon the civilian; even if the fact were true, the implied conclusion is plainly faulty; instead of being satisfied with reducing the mortality of our troops down to a level with the general mortality of the country, we ought never to cease working in this direction until we have brought it far below the general average. With the healthiest men to deal with, and all the appliances of the kingdom to carry out our dealings with them, it becomes as evident as any fact can be, that there must be some fault in our method of handling these men, so long as the deaths among them are not on a level with the lowest average in any class of men in the kingdom. Herein, we think, the report of the Commissioners is defective; it is satisfied with too little.

“Thus, the effects of drinking on the individual may not be such as to produce immediately injurious consequences. The slow chronic changes induced by continual imbibition of alcoholic liquors make themselves clearly manifest only when the mischief thereby induced is irremediable—until organic changes have occurred; and therefore it is not easy, in a rough way, to connect drunkenness with disease. But even in its immediate influence continual drinking must be most baneful, *by rendering the body prone at all times to take on diseased conditions, and also by rendering it less able to resist their destructive influence than when in a healthy state.* When is a person more susceptible to disease than at the time when his nervous and bodily powers are depressed after a debauch? To illustrate the state to which soldiers may be thus reduced, we may mention a fact related to us some years ago by one of the medical officers in the Guards. On asking him the reason why he attended the firing exercise of his regiment when practising early in the morning in Hyde Park—and this was just before the Russian war broke out—he informed us that he had received orders to do so, because so many of the men *fainted*. Naturally enough, we inquired the meaning of such an astounding fact—of Grenadiers fainting at morning drill—and discovered

that *drink* was at the bottom of the business. The men came into their sleeping-rooms half drunk, and turned out early in the morning without breakfast, or any desire for it, and just when the reaction of depression was setting in this fainting was the consequence. Now, if men's bodies are continually brought to this condition by drink, is it not self-evident that they must be at all times peculiarly prone to take on disease—any exciting cause provoking it? In other words, would the Guards be so readily the victims of pulmonary or other constitutional disorders when exposed to bad air and wet clothes, if their vital forces were not previously depressed and impaired by the agency referred to? An answer to this the Commissioners have not given. Of the large amount of drinking which is continually going on among soldiers, especially in large towns, any one may convince himself by watching the state in which they roll into barracks at night.

“The Commissioners have also slurred over, as before mentioned, in half-a-dozen lines that other fact—syphilis; but who is to believe that the cachexia induced thereby, the permanent injury to the constitution so constantly resulting from it, do not operate most banefully as promoting causes of other diseases? Some idea of the amount of venereal diseases in the army may be obtained from facts stated by us last year (*‘Medical Times,’* Feb. 13th, 1858). In 1848 the number of troops in the United Kingdom was 62,000; and among them the cases of venereal diseases were reckoned at 16,700. Are we to suppose that a disease, which affects the health of one fourth of the whole army, and compels the subject of it to remain on an average fifteen days in hospital, can fail to be indirectly a highly predisposing cause to the invasion of other diseases? And this especially when we recollect, as above stated, that syphilis is a disease which too often leaves its mark permanently indented on the constitution. There is also evidently another injurious consequence which arises from these diseases. If one fourth of the army is prostrated for fifteen days of the year by venereal diseases, the other three fourths have so much extra duty to do.

“Dr. Combe, of the Royal Artillery, who has had twelve years' experience of army diseases, tells us that chancre is more rife in the army now than it was formerly; that buboes follow it more rapidly and more frequently; and that secondary symptoms ensue in a larger number of cases. He attributes these facts mainly to the circumstance that the modern soldier is younger, and his condition inferior to that of the soldier of past years; and, therefore, that he is less able to resist the incursion of disease, and suffers more from it. Besides, he adds, that together with this diminished force of body has come a greater degree of physical labour to the individual. He admits that he has no statistics to back him; but these are the facts which have been forced upon him by his many years of experience.

“Venereal diseases have also, of late years, much increased in the navy, according to the statistical report just published by Dr. Bryson: “The average number of cases occurring annually for fourteen years in 1000 of mean force, did not exceed 74; whereas for the present year, 1856, it is more than double the amount, namely 168.”

“Now, if our opinion in this matter be correct, and we see not how it can be controverted on any sane medical grounds, then it follows that

the report of the Commissioners contains a very serious defect, and by its silence is productive of serious injury to the army. And in this way. If drunkenness and venereal diseases are of so little account as promoting causes of pulmonary and other diseases, then of course, we need not be so desirous of seeing them rooted out; and no strenuous efforts will be made in that direction: but suppose the Commissioners err, which we believe they do, then most assuredly it becomes a matter of the most serious interest to the authorities, not in a moral point of view alone, but for the health of the army and navy, to endeavour to alleviate and lessen the spread of these polluting diseases. Some attempt by the exertion of moral and sanitary influences should be made to arrest this evil, if merely for economical purposes. The loss of the services of 16,700 men out of 60,000 of our army, from syphilis, for fifteen days, may be readily reckoned. And we may conclude that the loss of service is just as great in the navy. The total number of days' sickness from venereal diseases in the *Brunswick*, with a crew of 500, during nine months, was 2441. In the navy, the extra duty thus thrown upon the rest of the crew must assuredly prejudice their health. Almost the whole of these venereal diseases are contracted in our chief naval ports. 'It is a fact,' says Dr. Bryson, "which deserves to be more generally known, that syphilitic diseases are more prevalent in this country, especially in the garrison and sea-port towns, than they are in any other part of the known world;" and he complains bitterly 'that the municipal authorities of those towns where it is most rife refuse to co-operate with the Government in establishing hospitals for the cure of the degraded creatures that swarm along the pavements.'"

ART. 5.—*Mortality among Miners.* By Dr. ———.

(*Lancet*, Dec. 4, 1858.)

"The vast population of miners form a body who dwell apart from their fellows, and make but rare demands upon their sympathy; yet ever and anon some terrible calamity, which overwhelms a whole community of them with the horrors of painful and premature death, fixes the attention of the rest of the world, and attracts their care. Apart from these sudden visitations, the miners suffer terribly from sickness and violent death. In 1851, the number of lives lost amongst 230,000 persons employed in the coal-mines of Great Britain amounted to 984; in 1852, to 986; in 1853, 957; in 1854, 1045; in 1855, 963; in 1856, 1027; in 1857, 1119. It is a sad total—7080 lives lost in seven years."

ART. 6.—*On the Duration of Life among the Jews.*

By Dr. E. GALTER.

(*Vierteljahrsschr., f. die Prak. Heilkunde*, Bd. ii, 1859.)

According to the investigations of Dr. Galter, the mean life of Jews is 46·5 years; of Germans, 26·7 years; and of Croats, 20·2 years. The mean life of Hungarians is not calculable for want of a table of births, but in Austria alone it is 27·5. The longer life of the Jew in different climates

is ascribed altogether to the influence of race. The following are some of the particulars contained in this paper:

In the county of Wieselburg, where Dr. Galter carried on his investigations for years, the deaths in every 1,000 during the first years of life were—

For the Jews	44.1
„ Germans	123.0
„ Hungarians	167.0
„ Croats	146.9

The deaths per thousand during the first ten years of life do not present these marked differences; thus the numbers are—

Among the inhabitants of Central Europe	.	448.8
„ „ of Prussia	.	468.0
„ Germans at Altenburgh	.	441.6
„ Hungarians at Methers	.	493.8
„ Croats at Gottendorf	.	566.2
„ Croats generally	.	500.0
„ Jews in the city of Wieselburg	.	462.0

For every thousand individuals, the age of 20 is attained by—

Jews	520
Germans at Altenburgh	513
Hungarians at Methers	445.8
Croats at Gottendorf	398.4

The deaths in every thousand between 20 and 29 at Frankfort are—

Jews	88
Christians	124

The deaths in every thousand are—

	<i>From 30 to 40.</i>	<i>From 40 to 50.</i>
Jews	66.7	70.1
Germans at Altenburgh	79.5	75.6
Hungarians	67.5	78.6
Croats	55.8	59.2
Inhabitants of Central Europe	.	72.3

At Frankfort the mortality per thousand between the ages of 30 and 40 is, for—

Jews	95
Christians	106

The age of 50 is passed, for every thousand, by—

Jews	319
Germans at Altenburgh	291
Hungarians	224
Croats	236
Inhabitants of Central Europe	297
Prussians	280

The age of 70 is passed, for every thousand, by—

Jews	123.1
Germans	87.8
Hungarians	54.4
Croats	71.1
Inhabitants of Central Europe	117.
Prussians	108

At Frankfort, the age of 70 is passed, for every thousand, by—

Christians	39
Jews	73

ART. 7.—*On the pernicious consequences of Intermarriage between near Relations.* By Dr. ———.

(*Dublin Hospital Gazette*, Dec. 1, 1858.)

The 'Art Médical' reprints from the 'Nouvelliste de Rouen' the following extract from an American paper:

"In the medical meeting which has just been holden at Washington, Dr. S. M. Bemis, of Kentucky, presented a very interesting report on the pernicious consequences entailed by marriage between near relations.

"The investigations made by Dr. Bemis have proved that 10 per cent. of the deaf and dumb, 5 per cent. of the blind, and about 15 per cent. of the idiots, found in the eleemosynary establishments of the United States, are the offspring of the marriage of first cousins. Out of 787 marriages between cousins german, as ascertained by Dr. Bemis, the latter has arrived at the conclusion that 256 had produced blind, deaf and dumb, or idiot children. Dr. Bemis's interesting and useful researches, on which the report is founded, were made in the state of Ohio. In the central counties, to which the census of 1850 ascribes a population of 1,528,238 souls, Dr. Bemis has discovered 483 marriages between first cousins. Of this number, 332 have been sterile, or have produced healthy children, whereas the 151 others have given birth to a sickly generation.

"Taking these particular data as the basis of a general table for the whole Union, of a white population of about 24 millions of souls, the following results would be obtained: 6321 marriages between cousins german, 3677 of which would produce infirm children in the following proportion: 1116 deaf and dumb, 468 born blind, 1854 idiots, and 239 scrofulous.

"Marriage between first cousins is infinitely more rare in the United States than in Europe; nevertheless, Dr. Bemis's report proves, at the same time, unhappily, their too frequent occurrence, and the sad results consequent upon them for the moral and physical condition of the children.

"Several states of the Union, Kentucky among others, have just passed an act, formally forbidding marriages between cousins german. A similar law, although at first sight it may appear to encroach on the rights of citizens, becomes in reality, in the presence of the figures we just quoted, a measure of social prudence and almost of humanity."

ART. 8.—*Experiments on the materials used for Military Clothing, as protective against heat and cold.* By Dr. COULIER.

(*Journal de Physiologie*, Jan., 1858.)

The result of these experiments is—

1. That the colour of the garment does not exercise any sensible influence upon the loss of heat.

2. That all the materials used for military clothing are capable of absorbing insensibly a certain quantity of moisture from the atmosphere, and that this quantity is greater in woollen than in linen or cotton. In cotton this quantity is least of all.

3. That this absorption of water does not involve any direct abstraction of heat from the wearer.

4. That the colour of the garment has great influence upon the absorption of solar heat, and that it is sufficient, with any kind of garment, to modify the surface accordingly, in order to realise the advantages presented by white garments, when the wearer was exposed to the rays of a hot sun.

ART. 9.—*On accidents occurring after Re-vaccination.* By M. H. LARREY.

(*Bull. de l'Académie Imp. de Méd.*, T. 23, 1859.)

M. Larrey, in a communication to the Academy of Medicine, gives an authentic account of the accidents which had followed the re-vaccination of certain artillerymen stationed at Toulouse. Sixty men were re-vaccinated on the 21st of June, with the usual precautions, from the arms of healthy adults. In nine of these serious accidents supervened, which were divisible into two categories, viz., general accidents of a typhus form, and local accidents of an erysipelatous form. The former occurred in three of the cases, and in these local accidents only appeared consecutively. In the other six the local symptoms appeared after, to be followed secondarily by fever. These cases were, however, no less serious than the others, for all the phenomena of severe phlegmonous erysipelas were developed. One fact is to be remarked, viz., that, with the exception of one, in all the cases, although vaccination had been performed in both arms, engorgement was only observed in the right arm. In appreciating the causes of this singular occurrence, M. Larrey adverts to the unusually high temperature, and to the fact that erysipelas was all that period prevalent in Toulouse. The artillery at that time too had to undergo unusual "fatigue," and it was ascertained that several of the "men had declined to avail themselves of the exemption of grooming their horses allowed for some time after re-vaccination." The force of this latter circumstance is seen in the fact that all the men were seized in the right arm, with the exception of one who was left-handed.

ART. 10.—On Re-vaccination. By Dr. VLEMINKZ, of Brussels.

(*Gaz. Hebdom. de Méd. et Chir.*, Nov. 12, 1858.)

The results of 1660 re-vaccinations performed by Dr. Vleminkz, in the prisons of Ghent and Vilworden may be stated in the following propositions:

1. Re-vaccination of well-vaccinated individuals generally yields very few useful results.

2. A person who has had smallpox should be more anxious to submit to re-vaccination than one who has been vaccinated.

3. Re-vaccination succeeds the better the more distant the time of the operation is from the original vaccination, or from an attack of smallpox.

4. Re-vaccination is useless up to the twenty-fifth year.

5. From that period up to the thirty-fifth year, re-vaccination yields useful results upon a certain number of individuals, but the number of the latter is exceedingly small; hence, such re-vaccination should not be pressed upon people, though medical men should not altogether discountenance it.

6. From the thirty-fifth year, re-vaccination becomes really preventive, and therefore necessary.

7. Supposing the operation had failed once, it is no reason for not trying it again some time afterwards, as nothing proves that receptivity has not returned between the first and the subsequent operation.

8. The re-vaccination of school boys or girls is useless.

9. Re-vaccination in armies organized like the Belgian (young soldiers) is also useless.

ART. 11.—On some of the cyclical Changes in the human System connected with Season. By Dr. EDWARD SMITH, Assistant Physician to the Hospital for Consumption, at Brompton.

(*Proc. of R. Med.-Chir. Soc.*, March 5, 1859.)

After stating that he has been occupied during the last four years in determining the cyclical changes in the respiratory functions, and referring to papers already published, and to very recent researches into the quantity of carbonic acid expired, and of air inspired, with the rate of pulsation and respiration in the cycle of the 24 hours, Dr. Smith shows that the daily cycle is not the same at all times, but varies from day to day and month to month through the cycle of the seasons. He also shows from the literature of the subject that, while the cyclical changes in any of the functions of the body had not been hitherto investigated, a large amount of knowledge had been gained in reference to respiration, but which was lacking in extent and demonstration. The results obtained by Mr. Milner, the talented Surgeon to the Wakefield Convict Prison, show that the prisoners gain weight from April to October, and lose weight in the other months of the year. The author has made experiments upon himself, aged 39, and upon Mr. Morel, a gentleman aged 48, and had continued them upon himself from March 31st, 1858, to the

present time. Both gentlemen were without hereditary or acquired disease, in robust health, capable of and accustomed to much mental and bodily exertion, above the middle height, and of very regular and moderate habits. The author takes every kind of food heartily, while Mr. Morel dislikes many. The time of experiment was from 7 to 8 a.m. before breakfast, with the body sitting and in perfect rest. His apparatus and method are described and exhibited. The results of the experiments show that as the season advanced from spring to the end of summer all the above-mentioned respiratory phenomena decline. The highest period is in the spring, the decline occurs at the beginning of summer, the lowest period is the end of summer, and the period of increase is the autumn. The extreme difference in the author is 30 per cent. in carbonic acid from the beginning of June to the beginning of September; and on the average of a month to the middle of August, is 17 per cent. in the carbonic acid, 30 per cent. in the air, and 32 per cent. in the rate of respiration. In Mr. Morel the loss at the middle of June is 27 per cent. in the carbonic acid and the air, and 28 per cent. in the rate. The author bears heat well, while Mr. Morel suffers much from it, and the results bear out a former observation by the author, viz. that those who bear heat badly have an excess of all the seasonal changes. Dr. Smith then averages the months according to these seasonal changes, and shows that there are two tolerably fixed periods (maximum and minimum), and two periods of change (increasing and decreasing), and tabulates them as follows:

Fixed—{ Maximum.—Jan. Feb. Mar. April, and May (sometimes Dec.)
 { Minimum.—July, August, part of Sept.
 Variable { Decreasing.—June (sometimes May.)
 { Increasing.—Oct. Nov. Dec. (sometimes Sept.)

He then shows the relation which these changes have to temperature, pressure of the atmosphere, and vapour in the air; and proves that the latter do not altogether account for them. The relation of temperature and pressure is an inverse one, and the former is very marked in sudden accessions, and is therefore a frequent cause of variation; but a medium degree of temperature at 55° to 60° , and a medium height of the barometer, at 29 to 30 inches, are accompanied by all the degrees of respiratory change. He then quotes Barrall's experiments showing the influence of season upon the ingesta and egesta, both of carbon and nitrogen, to prove that within certain limits variations in the amount of carbon exhaled indicate also similar variations in the nitrogen excreted. He also shows that as the skin had exhaled in July only six grains of carbonic acid per hour, in experiments upon himself, it was not important for him to refer to it. Dr. Smith then applies this discovery to the production of disease, and shows that the dangers of the fixed periods are from excess in both directions, *i. e.* excess and defect, and increase with the duration; while those of the variable periods result from the want of ready adaptation of the system to the variation of the external influences, and particularly of temperature and food, and would be the greatest at the commencement. He has abstracted the deaths in each season in the five non-epidemic years in London (1850—4) from diseases having periods of seasonal increase, and determines the excess

or defect in each quarter of the year from that which would have occurred if the deaths had been equally distributed over the year, and shows that there was a close correspondence between the states of the human system at different seasons and the type of disease then prevalent. Thus diarrhœa, cholera, plague, yellow fever, and asthenic diseases, with diseases of the bowels, prevail with the decreasing and lowest state of system, while diseases of the lungs and sthenic diseases prevail with the maximum state. He shows, further, that the advancing type of disease is that of the advancing season, so that in epidemics of scarlatina occurring after the minimum period the most asthenic type is observed at first, while in measles occurring with or after the maximum period the most inflammatory cases occur early. Scarlatina is checked by the increasing state of the system and measles by the decreasing. Hence in every disease it is important to bear in mind the season of the year as indicative of the state of the human system, and in every epidemic it is necessary to consider the nature of the advancing season. The author has also investigated the viability of children born at different seasons in reference to the period of procreation and of birth, the former illustrating the state of system in the parents, and the latter in the child, and found that it referred only to the latter. Of all the children who died under the age of 1 year in the northern district, from Newcastle to Kendal, in 1857, and whose age in months was recorded, the largest per-centage was born in the summer months, the period of decreasing and minimum vital action of the human system. Animals which procreate once a-year have their sexual appetites excited in the hot season, but they bring forth their young in the cold season. The paper concludes with four deductions:—1. Seasonal diseases must now be referred directly to the state of the system, and only indirectly to meteorological conditions. This does not affect the fashionable search after poisons. 2. The type of a seasonal disease varies with the advancing season. 3. The cyclical rotation of the seasonal changes in the system explains in great part the cessation of seasonal diseases; for while such diseases may increase as the state of system increases in which they arise, they must decline and cease as the state of system changes into its opposite. This is illustrated by comparing the march of a cholera epidemic from June to November, with the variations then proceeding in the system, and also the cessation of an epidemic of scarlatina and measles. 4. These cyclical changes are a part of the *vis medicatrix naturæ*. Having thus proved the great importance to health of this rotation of changes Dr. Smith exposes the folly of endeavouring to maintain in our hospitals, public offices, and houses, an unvarying condition throughout the year, and states that the contrary plan had been of incalculable value in the treatment of phthisis.

ART. 12.—*Illustrations of the mode of Propagation of certain Epidemic Disorders.* By Dr. T. HERBERT BARKER.

(*Brit. Med. Journal*, Nov. 20, 1858.)

Dr. Barker divides epidemic and endemic disorders into three classes:

Class 1. Diseases not transmissible, originating in a poison not reproducible in the body.

Class 2. Diseases transmissible, originating in a poison which is reproducible in the body.

Class 3. Diseases not transmissible, originating in meteorological variations.

To the first division of this classification, which it will be observed is based on the causes of the diseases, the author assigns ague, diarrhœa, remittent fever, and the endemic continued fever of this country, which, according to the writer, was ordinarily of the typhoid type. Of these diseases, the endemic continued fever was first considered in relation to its cause. Dr. Barker here adduces numerous illustrations from experimental and practical evidence, indicating that fever may be produced by poison emanating from decomposing animal substance and from cesspools. He infers that the poisoning producing the endemic non-contagious fever was inorganic and alkaline in character; and adds certain descriptions of the nature of cesspool-air to show that a peculiar alkaline emanation was always present.

He then considers diarrhœa as originating from the same cause, and very cautiously broaches the possibility that sulphuretted hydrogen is *the* purgative poison from the cesspool.

Several experiments as to the effects of sulphuretted hydrogen simply are adduced in support of this view. A great variety of illustrations, carefully collected and arranged from the observations of several practitioners, and bearing on the propagation of the diseases of this class, are introduced in this division of the paper.

In the *second* division, the chief point of novelty introduced rests on the arguments for and against the idea of the direct or indirect spontaneous origin of some of the communicable diseases belonging to the class; as, for instance, puerperal fever, typhus and typhoid fevers, and erysipelas. The author is disinclined to believe in the doctrine of direct spontaneity; but shows, by one forcible illustration, that a specific puerperal fever could be contracted by mere inoculation with the secretion from the peritoneum of a patient dying from simple peritonitis.

Smallpox, scarlet fever, measles, and true Asiatic cholera, admit, however, in the author's opinion, of no doubts in this respect. They clearly have their specific poisons, and could not be propagated without the actual presence of the poison. This division of the paper is also copiously illustrated by cases bearing on the points under debate.

In the *third* division, Dr. Barker includes common catarrh, croup, influenza, and a form of diarrhœa common among poorly clad people on a sudden fall of the barometer. Diseases of this class have this peculiarity: they break out all at once over a wide surface of country, and, after attacking on the same day, and even almost at the same hour, great numbers of persons, pass away in like general manner, and leave only their consequences behind.

Dr. Barker condenses his main argument in the succeeding propositions:

1. The poisons producing endemic non-contagious diseases may differ from those poisons which produce the contagious diseases in the simple particular, that the first-named poisons, however subtle and diffusive,

are inorganic, and lose their influence in the body which receives them; while the second are organic, and, being capable of reproduction under favouring conditions, are propagated in the animal body; finding, in fact, in the animal the conditions *most* favorable for their propagation and increase. These poisons, eliminated by the sick man and finding no favorable seed-ground in another person susceptible to them, may lie in temporary death and disease for a season. But once set at liberty and diffused by air or water so as to approach the susceptible individual, they put forth a new existence, and an epidemic starting from one centre is the result.

2. In regard to these poisons, whether of the first or second series, and whether organic or inorganic, if a medium for their transmission into the body be supplied, it is of little moment how the poisons are introduced. There has been much dispute lately as to air and water as the media of special diseases. The end of the dispute lies in accepting both as possible mediums, and in looking on the occurrence of one or the other as the medium as a mere matter of accident.

3. The last proposition he describes as relating to the influence of season, temperature, moisture, and other modifications of the atmosphere, in their effects on the spread of those diseases which have their origin in an organic and reproducible poison. Much has been written and said on this point, and, as is common, extremes of view have been taken. Meteorologists have denied contagion; contagionists have ignored meteorology. But whoever will remove from the combatants and look calmly on, will see that both have a kernel in their cracked nut. Given certain meteorological conditions in tropical India, and the vaccine virus, and even smallpox virus, loses its power. Given other meteorological conditions, and the virus has an activity which is inapproachable. Now, who in this matter is right or wrong? the palsied contagionist with the virus on his lancet-point which won't go, or the triumphant meteorologist with his eye immovable on the thermometer or the rain-gauge? Looking on without bias, the impartial observer sees them both correct in their special ways. He sees, in short, that the virus is necessary to the production of the disease; but that the virus can only act during conditions of heat or cold, dryness or moisture, favorable to its development. Finally, the author suggests that the same relationship between the poisons of spreading diseases and atmospheric conditions is universally sustained.

ART. 13.—*On the mode in which Arsenical Paper-hangings may prove injurious.* By Dr. ALFRED S. TAYLOR, F.R.S.

(*Medical Times and Gazette*, Jan. 1, 1859.)

A gentleman, whose library-walls were covered with an arsenical paper, had suffered for some time from chronic inflammation of the eyes, especially affecting the conjunctivæ of the eyelids. On the discovery that arsenic was contained in the green pigment of this paper in rather large quantity, he caused it to be removed during the summer, and to be replaced by another containing no arsenic. The inflammation from which he had suffered disappeared; but within the last few weeks it has returned. He informed Dr. Taylor that he had been dusting some books in a book-

case belonging to this room, and he supposed that the dust which had accumulated for two or three years had affected his eyes, and had caused a return of the inflammation. Some of the dust was carefully removed on Tuesday, the 21st of December, from the tops of a few books by a feather, and submitted to a chemical analysis. The dust weighed one grain and a half; it had an olive-green colour, and under the microscope it presented the appearance of fibres, with numerous particles of various colours, chiefly of a grayish-black. Treated by Reinsch's process, a portion of this dust yielded a deposit of arsenic, and there was therefore clear evidence that some of the arsenical pigment, formerly on the walls, had found its way through the glass doors of the book-case, and had been deposited in the form of a fine dust on the tops of the books. On Thursday, 23d December, after having made this chemical examination of the dust from a private dwelling, Dr. Taylor procured from the shop of Messrs. Marratt and Short, opticians, King William Street, London Bridge, a quantity of dust for the purpose of analysis. The walls of this shop are covered with an unglazed arsenical paper, and they have been so covered for a period of three years. In collecting the dust from the tops of the instrument-cases, great care was taken not to touch the walls. The quantity thus collected for examination amounted to about 450 grains. It was nearly black, and under the microscope it appeared to consist of fibres and sooty particles. It was very light and flocculent. 150 grains of the dust were examined by Reinsch's process, and enough metallic arsenic was obtained from this quantity to coat about ten square inches of copper foil, in addition to a piece of copper gauze. From the deposit on the latter, by the application of heat, octahedral crystals of arsenic were readily obtained. The cases had not been dusted for a period of nine months. The instrument-cases are secured by glass doors, and they are lined inside at the back with arsenical paper. A small quantity of dust was removed by a camel's-hair pencil from the projecting portions of the thermometers and barometers which are kept there. The quantity thus obtained weighed about eight tenths of a grain, of which five tenths were taken for examination. This half-grain of dust sufficed to cover with metallic arsenic a square inch of copper gauze. A portion of this, when heated, yielded a large number of well-defined octahedral crystals of arsenious acid.

These facts lead to the inference that the air of a room, of which the walls are covered with an unglazed arsenical paper, is liable to be charged with the fine dust of the poisonous arsenite of copper. Those who inhabit the rooms are exposed to the risk of breathing this dust. The poison may thus find its way by the pulmonary membrane into the system, or it may affect the eyes, nose, and throat by local action. That but few cases of actual poisoning under these circumstances have occurred is fortunate; but cases involving serious symptoms only, would be likely to attract attention. There may have been numerous instances of a disturbance of health depending on this arsenical paper, which from absence of suspicion, has been referred to other causes. The degree of exposure, the state of health, peculiar susceptibility, and the eliminating power of the system, may account for the comparative rareness of these cases. The mode in which the pigment is laid on the paper may be such as to prevent, in some instances, the fine particles of dust from escaping.

The fact, however, now demonstrated, that arsenical dust is breathed by those who occupy rooms thus papered, explains the similarity of symptoms observed, justifies the statements made by Dr. Hinds, Dr. Halley, and others, and proves that those who have experimented on this subject with negative results, have not taken the right course to arrive at the truth. Their results have, to a certain extent, misled the public by teaching them to rely on what is now proved to be a false security. If, as a general rule, the quantity of arsenic which can penetrate the body from this source is small, it is still desirable that arsenic should not be breathed, day by day, in any proportion. The defenders of this arsenical manufacture will hardly go to the length of asserting that this arsenical green, which is a potent poison in the stomach, can exert no injurious effect when taken into the lungs; and yet, unless this assumption be made, the inevitable inference is that these papers should not be used for covering the walls of our dwellings.

(B) ACUTE DISEASES.

ART. 14.—*On the changes which are supposed to have taken place in the type of Continued Fever.* By Dr. MURCHISON, Assistant-Physician to the London Fever Hospital.

(*Edinburgh Medical Journal*, Aug., 1858.)

The small mortality, and the frequency of sudden improvement in the symptoms, which were observed to follow venesection in the epidemic of 1817-20, and which have been attributed by Dr. Christison to that practice, were, as Dr. Murchison seems to show in this paper, characteristics of the relapsing form of fever which then prevailed, and have been equally characteristic of it at all times, even when bloodletting has never been resorted to. And if so, then it is not a legitimate argument in favour of a change in the constitutional type of fever, to contrast the mortality after bloodletting in the *relapsing* epidemic of 1817-20, with what would be the effect of bleeding in the *typhus* of the present day.

ART. 15.—*On the connexion between Cholera and Drought.*
By M. DE RUOLZ.

(*Lancet*, March 19, 1859.)

"M. de Ruolz," says the writer of 'Medical Annotations' in the '*Lancet*,' "well known for important discoveries in the art of electro-gilding, has been content to argue from a narrower basis, and has lately communicated to the 'Cercle de la Presse Scientifique' of Paris, an interesting series of facts regarding the proportion of moisture contained in the atmosphere during the prevalence of cholera. By analysing the various statistical data collected during the French epidemics of 1832, 1849, and 1854, M. de Ruolz has deduced the following facts:—In 1832, the epidemic in Paris reached its height in April, when the hygrometer was lowest: it declined to the utmost in September, when the hygro-

meter was highest. In 1849, the hygrometrical observations at the observatory of Paris had been unaccountably neglected; but 1854 afforded results quite in accordance with those of 1832. Hence, M. de Ruolz infers that there undoubtedly exists a positive coincidence between the intensity of the epidemic and the hygrometrical state of the atmosphere, the former being in the inverse ratio of the humidity of the air. Other circumstances he considers to point to the same conclusion: thus, Lyons, a city remarkable for its damp atmosphere, owing to the two rivers which embrace it, has never been visited by cholera. Amongst washerwomen, who live in a damp medium, he says that cholera has always been very low, and he makes the same assertion with reference to 'persons living in damp places, on the banks of rivers, &c.' Finally, M. de Ruolz tells us that, during the last choleraic invasion in London, the copious watering of the streets was found very beneficial. The views thus enunciated in the 'Cercle Scientifique' do not remain wholly unopposed. Thus, it is remarked that sailors were very subject to cholera; that cholera made great havoc in Holland, where the air is notoriously moist; and that in certain localities the cholera has been known to lay waste one bank of a river and to spare the other. M. de Ruolz, however, presses for further investigation, and suggests that, by way of experiment, in any future epidemic the streets should be well watered, and the fire-engines should play on the roofs of the houses in the infected quarter. It were idle to smile at his singular expedient, if indeed it were probable that any useful result could flow from it. And there is no valid reason why Mr. Braidwood should not brigade his force against an epidemic, or why cholera should not be attacked with the fire-engine as well as with the lime-pail and the brush of the whitewasher—a favorite panacea with metropolitan vestries,—or by the artificial creation of ozone and the introduction of certain ozonified breezes, as more subtle chemists have recently suggested. But we have the strongest doubts whether M. de Ruolz's theory will 'hold water.' The experience of Lambeth, of Wandsworth, and of other humid districts close to the river-side, has certainly not offered confirmatory facts; and though unacquainted with the actual statistics of deaths from cholera amongst London washerwomen, we are in possession of a number of isolated observations which do not dispose us to regard soapsuds as a prophylactic against epidemics in the sense which M. de Ruolz suggests."

ART. 16.—*Smallpox supervening on Measles and Scarlatina.*

By Mr. BROOKE GALLWEY, Surgeon, Royal Artillery.

(*Lancet*, Aug. 28, 1858.)

In one of these cases we have smallpox and measles co-existent in the same individual; in the other, smallpox and scarlatina—a coincidence of which few, if any, instances are on record.

CASE I.—"Between three and four years ago, a young soldier, declining rapidly in the second stage of phthisis, became the subject of measles so well developed, that it might have been selected as a model case from which to study that exanthem. The eruption had arrived at its climax and had gained

its turning point,—the thoracic symptoms, as might be expected in such a subject, being exceedingly severe,—when the patient sustained unexpectedly an attack of rigors, and, at my visit on the following morning, presented, *quoad* his entire face, the appearance as of a substratum of shot beneath the skin. Unprepared for what was about soon after to develop itself, it will easily be understood how much at a loss I felt to account for the very unusual state of things thus unexpectedly presented to my notice. Twenty-four hours later and I found I had a case of *confluent smallpox*, engrafted on a ground of rubeola, to deal with; and I may truly say that never had I met with more finished representations of either disease than were now delineated together in the same individual. The patient contended manfully against the three separate and formidable assailants that had thus piteously set upon him, until, under the maturing process of the last, the powers of life gave way, and a fatal issue overtook him.

“The case, of which I have now afforded but a brief outline, occurred at the Military Hospital at Devonport, and was seen, amongst other individuals, by the principal medical officer of the station, Mr. Dartnell. I forwarded the details of it at the time, *in extenso*, to the Director-General of the Medical Department of the Army, in the archives of whose office I doubt not they still exist.”

CASE 2.—“Within a few weeks of the coincidence I have recorded,—smallpox, I should explain, being *prevalent* at the time in the establishment,—the following not less remarkable occurrence developed itself in the same hospital in the person of a soldier of the 1st Somerset Militia:

“Private James Bale (or Ball), æt. 20, after some few days of pyrexia, accompanied at first with ‘a strawberry’ tongue, and then with so much swelling of that organ as to demand free incisions,—severe sore-throat being an attendant symptom,—presented, in due time, an eruption of scarlatina, which came out well, and was intense in degree. The pulse ran high, deglutition became very difficult, delirium set in early, and the general tendency of the symptoms was in a downward direction.

“Between the third and fourth day after the eruption had declared itself, the following entry occurs in the medical register of the regiment, for which I am indebted to the (then) assistant-surgeon of the corps:—‘Some vesicles, depressed in centre, have appeared, bearing the character of varioloid eruption; is with difficulty aroused to answer questions; the primary scarlatinal eruption is scarcely perceptible; tongue very dry and coated—almost black; fæces passed under him in bed.’ Evening of the same day: ‘Face thickly covered with *smallpox pustules*; scarlatinal eruption receded; is in every respect worse, and apparently sinking.’ And, the day after, ‘Passed a quiet night, but lies in a comatose state; varioloid eruption in its most confluent form; breathing laborious; cheeks puffed out in expiration,’ &c. The patient soon after expired.”

ART. 17.—*On the question of the affinity between Scarlet Fever and Measles.* By Dr. KÜTTNER, of Dresden.

(*Journ. für Kinderkrankh.*, and *Dublin Hosp. Gaz.*, Dec. 15, 1858.)

That measles and scarlatina, in their symptoms and in their essence, present two well-defined states of disease, is one of the most indisputable facts in pathology. Schönlein, making use of an analogy derived from botanical science, looks on measles as a peculiar exanthematic form of catarrh, and accordingly places it in the family of the catarrhs; whereas

scarlatina is placed by him in the group of erysipelatous diseases. Admitting that, in the regular course, such sharply defined examples of the diseases exist, Dr. Küttner observes, that experience furnishes us with androgynous cases calculated to embarrass even the experienced "diagnostiker." If the diseases were always so well marked as we find them in the handbooks, there would be no difficulty. Sometimes the exanthem bears the character of scarlatina, while the catarrhal irritation of the bronchial membrane, and of the mucous membrane of the nose and of the eyes, indicate the morbillous process; or, on the other hand, the rash of measles is accompanied by vomiting, by a sharp attack of angina, and by the characteristic scarlatina tongue.

We see occasionally, in the same individual, parts of the skin presenting the scarlet-red eruption, while in others the rosy rash of measles exists. We have, then, not merely examples of transition, but we see cases which may be termed hybridous. If we were even disinclined to assign any relationship to the two diseases, arising from the fact of their passing into each other, there is still another observation which shows it in a higher degree—namely, that the *same* contagious matter appears capable of producing, in different individuals, different diseases: in some measles, in others scarlatina. Many reliable proofs of this are to be found in medical literature, to which the following may be added, as affording good examples. During an epidemic of measles, a boy of sixteen years of age became affected with the disease, which ran a favorable course, and at the end of three weeks he was sent from Dresden to his father's country house, at some distance from the city. A little sister, two years of age, who visited the brother on the day of the appearance of the rash, although immediately sent away, sickened on the tenth day, presenting the ordinary symptoms of the eruptive fever; in her, too, the disease passed over mildly. A second sister, one of the older members of the family, who had before repeatedly attended upon persons in measles without taking the disease, did not now escape. There yet remained a third sister, who was married, and who had been for fourteen days on a visit to her father's house, but who, from never having had measles, carefully avoided intercourse with the sick and the convalescent, not, however, guarding against the possibility of transmission through a third person. Without any previous indisposition, sharp febrile symptoms appeared in this case, leading to the belief of the invasion of measles. An intense scarlatinal eruption, however, manifested itself, with the characteristic affection of the throat, and with the red tongue. The exanthematic period passed over without any remarkable symptoms, ending in the second week with an extensive exfoliation of the epidermis. No second case of measles or scarlatina occurred in the house.

Although observations such as these have been made by practical physicians, and recorded, still this is worthy of notice, from the concurrent circumstances, and because the course of the disease was so characteristic as to leave no doubt as to the correctness of the diagnosis.

In what relation scarlatina and measles stand to each other, and whether the same contagion can, in reality, produce both forms of disease, we are scarcely in a position to determine. A sceptic will naturally

say that the scarlatina of the last-named case was not the product of the contagion of measles, but was developed independently.

Admitting, however, that a direct proof to the contrary cannot be given, the fact nevertheless remains, that no communication from without, capable of conveying scarlatina, took place. Is it in reality so preposterous a notion, that the matter of the exanthematic contagion may act like a ferment, without any specific character, and according to individual disposition, may produce measles in one case and scarlatina in another, as, under similar conditions, the impression of cold may cause in one individual catarrh, and in another rheumatism?

ART. 18.—*On the use of Hydrochloric Acid in Scarlatina.*
By Dr. M'SHERRY.

(*American Journal of Medical Science*, Oct., 1858.)

Dr. M'Sherry says that he has for some years past employed muriatic acid in doses of from one to three drops largely diluted with water. It is readily taken, and by its cleansing the throat in its passage, supersedes the necessity of gargles. "I give my little patients oranges to eat, and lemonade to drink, keep up faithfully a surface inunction, and use habitually little other medicine than the acid. It may be necessary to clear out the primæ viæ by an emetic or aperient, or both; but I have long given up the use of *purgatives* as not only improper, but very dangerous. A single dose of calomel may be well at the beginning, but no acids should be given with it, or even follow it speedily. The danger of the association of vegetable acids with mercury is not so obvious, and may be overlooked. But it has happened that they have converted calomel into a more dangerous form of mercury; and a fatal case is recorded merely from its having been given in that popular vehicle, currant-jelly. The practical physician should not let such an instance as this escape his memory."

ART. 19.—*On the supposed antagonism of Ague and Consumption.*
By Dr. PEACOCK, Assistant Physician to St. Thomas's Hospital.

(*Medico-Chir. Review*, Jan., 1858.)

After a careful statistical investigation, Dr. Peacock concludes that the information elicited in this manner is only negative. He then inquires whether clinical observation is capable of yielding more positive results, and after relating six cases, he proceeds to say—

"The facts which I have now detailed, conclusively show that neither does the existence of consumption prevent the occurrence of ague, nor the occurrence of ague preclude the subsequent development of consumption; but they do not prove that the supposed antagonism may not, in some degree, exist. For, on the one hand, phthisical patients, subjected to the influence of malaria, may take ague in less proportion than healthy persons similarly exposed; and on the other hand, persons who have had ague may be less liable to consumption than those who have not had the disease. These objections I have no means of meeting, but when it is

considered that, within a period of two years, five cases of the co-existence of the two diseases have fallen under my own notice, at one public institution, where the majority of phthisical patients do not come from malarious districts, the coincidence cannot be regarded as exceptional or rare.* I cannot, then, but conclude, that it is not probable any material antagonism exists between phthisis and intermittent fever. The facts do not, however, warrant the denial of the supposition altogether, and there are probably few popular ideas which have not some foundation in truth.

"The correct inference would appear to be, that the influence, if any, exerted on the prevalence of consumption, by a malarious atmosphere or by ague, is far less important than that of various other causes which affect the development of that disease. The practical conclusion also to be arrived at is, that as phthisical patients may take ague, and as such complication materially aggravates the original disease, we should, in selecting residences for consumptive patients, avoid those situations which are marshy, or in which aguish affections are known to prevail."

ART. 20.—*On Rheumatism.* By Dr. SKODA.

(*Prager Vierteljahrschr.*, Bd. iii, 1858; and *Edinburgh Medical Journal*, Jan., 1859.)

Formerly, it was the practice to treat acute rheumatism with warm applications, from the dread of driving the disease inwards: now, we know that the affection of the internal organs is so far from being in any way opposed to that of the joints, that, on the contrary, the lungs, pleura, peri-, and endo-cardium suffer most when the joints are most severely affected, though to this there are exceptions. From this it follows that no ease-producing applications are to be shunned from any vague dread of metastasis. Should warm applications give the desired relief, there can be no possible objection to them; but if not, as often happens, then we may at once with propriety resort to cold ones; and Skoda is satisfied that, by doing so, lung and heart disease are not increased, but greatly diminished, particularly if, under their use, the pulse diminishes in frequency, and the patient's restlessness decreases, as these are all the alleviations attainable under any therapeutics in rheumatism. As to other remedies, Skoda has but little to say in praise of them. When the joints are much swollen, leeches are not contraindicated, but the relief obtained is not lasting; bloodletting (general) may also be occasionally usefully employed, but its repetition is injurious. Of internal remedies, Skoda has employed Sod. Bicarb., Iodine, Sublim. con., Quinine, Tart. emet., Digitalis, Pot. nit., Ammon. mur., &c., without much benefit. Quinine is only useful when the attacks are paroxysmal. Digitalis and Tart. emet. are, however, occasionally useful in individual cases, though no distinct reason for it can be given, nor their success predicted. Narcotics are most useful in lessening the pain; and as this usually increases

* During the two years in which these five cases occurred, Dr. Peacock treated, at St. Thomas's Hospital, among the in- and out-patients, 236 cases of ague, and at least 262 cases of phthisis. The precise number of cases of the latter disease he cannot give, as some cases of phthisis are entered in the out-patient's book as "affections of the chest." He has also met with several other cases in which there was reason to suspect tendency to phthisis in aguish patients.

at night, they are best given at bedtime; unfortunately, many bear these ill, small doses being followed by sickness, vomiting, and increase of pain.

(Dr. Kraft adds, that for many years ice applications have been successfully used, in his department of the hospital at Prague, in the treatment of acute rheumatism, particularly when great pain is accompanied by greatly increased temperature of the parts.)

ART. 21.—*Novel propositions in Gout and Rheumatism.*

By Dr. FRED. J. BROWN, of Chatham.

(*British Med. Journal*, Nov. 6, 1858.)

Dr. Brown embodies his views respecting gout and rheumatism in the following propositions :

I. *Gout and rheumatism never co-exist.*

That is to say, there is no such disease as rheumatic gout.

This is the opinion of Dr. Garrod and of an increasing number of physicians. The disease termed rheumatic gout is really gout affecting the large joints. It is commonly met with in licensed victuallers, and it has a duration of four months.

II. *An individual with the gouty diathesis never at any period of his life has rheumatism : and conversely, that an individual with the rheumatic diathesis never at any period of his life has gout.*

This proposition is put forward as original, and requires proof : it is on its trial.

The author's belief is, that every pain commonly termed rheumatic occurring in an individual with the gouty diathesis is in reality a gouty pain : and that gnawing pains in muscles, membranous layers, and joints, arising from exposure to damp, are properly to be termed gouty in gouty individuals, and rheumatic in rheumatic individuals. To discover the diathesis, it is necessary to learn the family history, hereditary influence being clearly marked in gout. There are indications also which enable us to conclude that an individual has the gouty diathesis, such as red gravel and renal calculi, tophaceous deposits in the outer ear and elsewhere, one form of dyspepsia, inequality of temper, habitual condition of the urine, &c.

Women whose fathers have suffered from gout commonly present the minor symptoms of the same disease; but these symptoms are very often regarded as appertaining to other complaints, and the pains are termed rheumatic. In such women, bloody urine and renal calculi are frequently observed.

III. *Gout affects the system occasionally as an acute general fever, like acute rheumatism (which is termed by the public rheumatic fever).*

Thus there is a podagric or gouty fever, like as there is a rheumatic fever. If there be any objection to the term fever in these cases, let the term acute general gout be employed, to correspond with the term acute general rheumatism.

Those that choose to designate the complaint fever, are justified in doing so by the analogy of catarrhal fever and of biliary fever—in both which affections there is toxæmia and nervous disorder, even as there is in rheumatic fever and in gouty fever.

Many cases now termed acute rheumatism or rheumatic fever are really cases of gouty fever. They are to be distinguished by the discovery of the diathesis, by the presence of uric acid in the serum of the blood, by the duration of the complaint, and by the effects of remedies.

IV. *The duration of gouty fever bears a triplicate ratio to that of rheumatic fever.*

Thus, whilst rheumatic fever has a full duration (in severe cases) of forty days, gouty fever endures one hundred and twenty days.

There are many mild cases of rheumatic fever which last only twenty days; and physicians have reported cases yielding to treatment in nine days. The writer does not know whether there are corresponding cases of gout; but he has never seen any subside under four months.

ART. 22.—*Remarks on the use of Bleeding in diseases.*

By Dr. MARKHAM, Physician to St. Mary's Hospital.

(*Proc. of Roy. Med.-Chir. Soc.*, Nov. 30, 1858.)

The object of the author is to show, that by arguing from certain admitted facts respecting the effects of bleeding, a rule of practice may be deduced indicating the right application of the remedy in diseases. The conclusions at which he arrives are these:

1. There is no proof that venesection has any *directly* beneficial influence over the progress of inflammations, either *external* or *internal*. On the other hand, the injurious effects of large bleedings, especially in those inflammations in which the integrity of the lungs is seriously compromised, have been often demonstrated.

2. Nevertheless, venesection is, at times, of great service *indirectly* in the course of inflammations, and of all other diseases which occasion congestion and oppression of the heart, by removing this *secondary* consequence, which arises accidentally out of the inflammation.

3. In all cases in which venesection is of service, it acts alike—viz., by relieving the cardiac congestion; it neither arrests nor modifies beneficially the inflammatory process.

4. A marked distinction is to be drawn between the effects of bleeding in inflammations and the local abstraction of blood from an inflamed part. Local abstraction of blood materially influences the inflammation, reducing the most characteristic of its phenomena—the pain, the heat, the redness, and the swelling; but it only influences, in this way, *internal* inflammations when there is a direct vascular connexion between the part inflamed and the part whence the blood is drawn:

5. It is not denied that local irritation of an external part may influence an internal inflammation (even when there is no direct vascular communication between the skin and the inflamed part) by reflex action,

conveyed thence from the skin through the vaso-motor nerves of an inflamed part.

The author demonstrates the inefficacy of venesection over internal inflammations in two ways: 1st, by arguing of what is *seen* of its inutility in external inflammations; and, 2dly, by the fact of the large and general concurrent testimony of modern experience, which has proved that large bleedings—the only bleedings which have any manifest influence over inflammations—are often very injurious, their good effects being dubious and disputed.

Venesection has been long since abandoned in the treatment of external inflammations because of the danger and inutility of the practice; and though less easily traced in the case of internal inflammations, the same conclusion has gradually forced itself on the minds of observers. The practice is no longer regarded as essential in their treatment; but the author cannot believe that physicians have been during so many ages, and still are, acting under a delusion as to the services rendered by venesection in internal inflammations. He, therefore, endeavours to explain the discrepancy by assuming the position—that venesection, as regards internal inflammations, is of service, not through any *direct* influence which it exercises over the inflammatory process, but in consequence of its removing certain of the *secondary* consequences which arise accidentally out of the inflammation—to wit, the oppressed and congested condition of the heart. He asserts that venesection is never required excepting when this congestion of the heart exists; but at the same time observes, that there are congestions of the heart, and periods in the course of all congestions, in which no relief can be hoped for from the remedy. Modern experience justifies this position, for venesection is rarely ever practised now, except in those diseases in which this congested condition of the heart necessarily plays a prominent part. The benefits, indeed, of venesection become more clearly manifest in proportion as the disease for which it is practised produces a higher degree of this congestion. As illustrations of this, cases are related in which the original disorders provoking this congestion of the heart lay respectively in the heart itself, in the lungs, in the abdomen, and in the brain. In all of them the same condition of the heart and the same symptoms were present, claiming a similar treatment. The relief given by venesection, in three of these cases, was immediate and permanent; in two of them no inflammation existed; and in the third—one of pneumonia—the venesection had no influence over the *inflammation* of the lung, for the stethoscope demonstrated that the portion of lung inflamed was in the same condition of consolidation the day after as on the day of the bleeding; in the fourth case—injury of the head—the man was not bled, and died, the immediate and only apparent cause of death found being extreme congestion of the heart and lungs. Bleeding, it is believed, would have saved this man's life.

He offers the same explanation of the benefit of venesection in wounds of the lung, long before inflammation exists, and he thinks that the same circumstances explain the relief of the pain often attendant on pneumonia, which, while occasionally due to pleurisy, he thinks more frequently produced by cardiac congestion—a pain which is sometimes felt when there is no pleurisy, or may not be felt when pleurisy is

present. In certain conditions of disease of the heart and great vessels, in injuries of the head and apoplexy, and even in peritonitis, the benefits occasionally following venesection may all, he thinks, be referred to the relief of cardiac congestion. Such an explanation seems to the author clear and simple, and in complete accordance with our physiological knowledge and our practical experience; and with reference to any other beneficial and direct actions, which venesection is supposed to exert over inflammation, he observes that all our knowledge of the effects of venesection has not yet enabled us to show what those other actions are; and all our modern experience manifestly tends to prove that venesection has no directly beneficial influence over inflammations, but that, if large, it acts injuriously by weakening the system, which has to sustain the force of the inflammatory process.

If the facts here maintained be correct, then it necessarily follows that the objects of, and indications for, venesection become clear and definite, and that a rule of practice may be established from their consideration. It also follows that venesection is now-a-days less frequently practised than is desirable; that it must have been of service in other days, just as it is of service now; that it is requisite now, just as it was requisite then.

The author next refers specifically to venesection in pneumonia. Here there are two main special facts to be considered, which contra-indicate to a certain extent the venesection: 1st, the diseased condition of the lungs, which produces the cardiac congestion, cannot be removed by the bleeding; 2d, the loss of blood is, so long as the pneumonia lasts and in proportion to its extent, an irreparable loss. Hence it follows that the more extensive the inflammation, and the more urgent the symptoms, the greater is the danger of venesection; and in fact, just in proportion as the bleeding is more required to relieve the heart, is the practice of it less applicable. In pneumonia the function of the chief sanguificating organ of the body is arrested; and therefore to take away blood at such a time is to take away what cannot be restored so long as the inflammation lasts. The loss of blood, which might be borne with impunity in other inflammations, seriously compromises the future of the patient in this inflammation of the lungs. Venesection, he says, is applicable in pneumonia when the general symptoms have arisen rapidly and are severe, and when the inflammation is limited, as in the case related—when the urgency of the symptoms is, so to say, out of proportion to the extent of the inflammation, as measured by the stethoscope; that is, when the aërating processes are not seriously and extensively compromised. The object of the venesection is, in all cases, to relieve the heart from its temporary embarrassments. When the congestion of the organ is the consequence of its own partial paralysis, then of course venesection cannot restore to it its equilibrium.

With reference to the local abstraction of blood, Dr. Markham points out the importance of vascular connexion between the skin and the inflamed organ, reasoning from the analogy of external inflammations. The benefit of leeches in pneumonia he refers simply to the attendant inflammation of the parietal pleura, and thinks that in pericarditis the pain is frequently due to concomitant pleurisy, and is relieved in the same way. In endocarditis he believes they can be of no service; nor

can they draw one drop of blood from an inflamed liver or kidney. In such cases he conceives that the benefits attributed to cupping or leeches may be due to other remedies employed at the same time.

The author makes no attempt at any explanation of the mode of action of either venesection or local abstraction of blood. If the facts stated be true and rightly interpreted, their practical deduction may be accepted, without waiting for any theoretical explanation of them.

ART. 23.—*An argument for Antiphlogistic Treatment.* By Dr. HENRY KENNEDY, Physician Extraordinary to Sir P. Dun's Hospital.

(*Edinburgh Medical Journal*, Jan., 1859.)

In a paper 'On the Change of Type Theory in Disease,' in which it is maintained that the type is always changing, and necessitating in consequence corresponding changes in treatment, we meet with the following passages in favour of antiphlogistic measures :

"Have those, however, who are now so ready to assert that nature is the curer of disease, ever fully considered her plans? I doubt it; else, it strikes me, they would not be so ready to find fault with the antiphlogistic treatment. For then they must have observed that nature's operations have ever a tendency—supposing them to go on to cure—to reduce the system. Take an affection of very common occurrence, and more frequently left to itself than, probably, any other—a common cold; and what do we observe? A general *malaise*; some loss of appetite and strength; a disinclination to follow one's usual pursuits; occasionally some slight loss of flesh; very commonly paleness of the face, and then recovery. But if we go a step farther, and suppose a case of fever, left to itself, what will be observed? An utter loss of strength, appetite, sleep, and flesh (in cases where the latter is not observed, a very unfavorable sign it is); a persistence of disease for two, three, or more weeks; often during this period the occurrence of hæmorrhage, of one form or other, more frequently from the nose than elsewhere, and much more frequent at certain periods than others; the occurrence of diarrhœa, quite independent of the typhoid type of fever; sweats; and, lastly, crises of different kinds. I have supposed the fever not to have been complicated at all. But who does not know how rare this is; and that the disease is ever presenting complications of the head, chest, or abdomen? One point, at any rate, is beyond all dispute, that the final result leaves the patient much reduced in every way; and, speaking generally, some weeks elapse before perfect recovery takes place. In other words, nature's cure is by direct and unmistakeable antiphlogistics. But now-a-days, and when nature is made all in all, these facts, though so obvious, appear to be entirely overlooked; and yet I cannot but think they prove, most unequivocally, the direction which art should or may take; and that the present cry out against antiphlogistics is not only utterly groundless, but that this line of treatment may be followed with the greatest advantage, when the case requires it."

(C) CHRONIC DISEASES.

ART. 24.—*On the treatment of chronic Alcoholic Intoxication.*

By Dr. MARCET, Assistant-Physician to the Westminster Hospital.

(*Lancet*, April 2, 1859.)

Oxide of zinc, according to Dr. Marcet, is the proper remedy for the nervous symptoms resulting from the long-continued use of alcoholic beverages. After considering the physiological and therapeutical properties of this drug, and stating that he had seen instances in which it has induced drowsiness, and even sleep, the author analyses twenty-seven cases, and gives a synoptic table in which the following points are noticed:—1. Name, &c., of intoxicant consumed. 2. Quality and quantity of drink taken. 3. Period of existence of intemperance. 4. Sleep. 5. Hallucinations. 6. Trembling. 7. Other nervous symptoms. 8. Pre-existing disease. 9. Result of the treatment. Ten of the above cases are taken in detail, the results showing the efficacy of the oxide of zinc as a remedial agent. With respect to the treatment, it is not merely necessary for the patient to cease drinking, as the symptoms referable to the nervous system often occurred long after the habits of intemperance had been abandoned, but an active treatment is also absolutely necessary. The oxide of zinc is given in doses of two grains twice a day, in the form of powder, an hour after each corresponding meal. The dose was generally increased in the ratio of two grains every three days, until the patient took six or eight grains twice a day. Thus sleep was soon induced, the trembling of the body and limbs rapidly disappeared, the patient no longer suffered from headache or giddiness, and the hallucinations vanished; and in the course of from three to six weeks the patient had recovered from a long and painful illness. The weakness, a common symptom accompanying the disorder, was very difficult to overcome, and often continued a long while after the individual was quite well in every other respect. Another fact noticed was the complication of chronic alcoholic poisoning with bronchitis and rheumatism, in which cases the effects of the oxide were less marked; and in these instances the functional disturbance of the nervous system often gave way without any improvement in the coexisting disease. Accordingly, in these cases the author added to the treatment as soon as the effects of the oxide were exhausted. The results of the twenty-seven cases are as follows: Six continued attending; eleven had been discharged, cured; four left the hospital (Westminster), quite recovered; four, much improved; and two ceased attending after the first or second visit. "In cases of chorea, mild hysteria, paralysis, and lead palsy," Dr. Marcet adds, "the use of oxide of zinc gave but unsatisfactory results, and in the majority of cases of epilepsy it could not be considered an effectual remedy."

ART. 25.—*On Syphilitic inoculation.*

By Mr. HENRY LEE, Surgeon to the Lock Hospital.

(Lancet, Jan. 29, 1859.)

Is it true that a primary syphilitic sore will invariably give rise, through inoculation with its secretion, to a *characteristic syphilitic pustule*? M. Ricord answers this question in the affirmative; Mr. Lee in the negative.

"In the year 1856," says Mr. Lee, "I published an account of some cases in which, notwithstanding the assertions so unreservedly made, and so generally received, I had not been able to produce any effect by ordinary inoculation. The experiments were tried in this hospital, care being taken to select instances in which the sores had not begun to heal; and amongst the cases were the following :"

Thomas C—, æt. 16, was admitted on the 27th of November, with an indurated sore, extending half-way round the margin of the prepuce, causing phymosis. This had commenced a fortnight previously. The secretion from the sore was carefully inoculated on the patient's thigh in several points. The inoculations were followed by no result, and the patient was soon afterwards affected with secondary syphilis.

James G—, æt. 25, had a large indurated sore near the orifice of the prepuce, which had appeared as a pimple four weeks previously. The secretion was carefully inoculated in several points, but without result. Constitutional syphilis followed.

Matilda P— presented a well-marked indurated sore on the left external labium, which she stated had existed one week only. The secretion, which consisted of a thin serous fluid, was carefully inoculated. No result followed the inoculation.

Julia B—, æt. 21, had a red, glazed sore on the external labium, surrounded by distinct specific induration. The disease had commenced three weeks previously as a pimple. The secretion from the surface of the sore was inoculated without result.

"Since the period above referred to, we have inoculated the secretion from a considerable number of sores presenting the characters of the specific adhesive inflammation; and as a rule, no result has been obtained where no artificial irritation had been applied. The secretion, in these cases, has consisted chiefly of epithelial *débris* floating in serum, more or less turbid. The character of the secretion of these sores may readily be altered by anything that is brought in contact with them. The application of caustic, or a thick scab which confines the secretion, or a piece of linen which sticks to the sore, will produce a temporary discharge more or less puriform; but dress the part with wet lint for a day or two, and the natural character of the discharge will again be evident.

"As a rule, then, I say—at least that has been our experience at this hospital—that this kind of primary sore is not, under ordinary circumstances, capable of being inoculated with the lancet; and we, therefore, cannot but come to the conclusion, that those who have maintained that all primary syphilitic sores can be alike inoculated, have generalized too hastily."

But a much more important question remains—namely, does it follow that because these sores are not capable of being inoculated by the lancet, in their ordinary condition, that therefore they cannot be communicated at all? And, if capable of being inoculated, under certain circumstances, are the results produced identical with those produced by the inoculation of the secretion from suppurating sores?

“In order to determine these questions, I would request your particular attention to some cases now in the hospital, and particularly to that of a boy named A—. The details of this case, and the results of the inoculations performed, I will immediately mention; but before doing so, I would draw your attention to the fact, that in practice, we continually meet with cases which, in their early stages, are similar to the one I am about to describe, and which, with or without treatment, become covered by epithelium, and in which, although the specific induration remains, yet the surface affords no fluid secretion whatever. Inoculation, in the ordinary way, at this period of the disease, would be entirely out of the question. But if a person in the condition I have mentioned gets married, his wife will have a very good chance, even before her pregnancy, of becoming infected with syphilis. Some very distinct cases, where this has happened, have fallen under my own observation. How, then, is the disease communicated in these instances? We have here a rather numerous class of cases in which the sores have become covered over with cuticle, which yield no pus or fluid secretion of any kind, and yet which are capable of communicating infection from one individual to another. How, I ask, does the inoculation take place from a sore affected only with the specific adhesive inflammation, either before or after that sore has apparently healed? Some light will, I think, be thrown upon these questions by the careful consideration of the facts presented to our notice in the boy A—, at present in the hospital.”

This lad had had gonorrhœa six months before his present attack, but otherwise he had never had any venereal symptoms. His present disease was of about a fortnight's duration. He first perceived a superficial sore behind the corona glandis, which healed in a few days. Two or three days after the first appearance of this sore, a little pimple appeared on the outer skin of the prepuce. This was squeezed, and discharged a watery fluid. A sore then formed, which continued to increase. He applied to me on the 26th of July, with a well-marked Hunterian chancre. This was of a circular form, surrounded by well-marked and accurately-defined induration, and discharged a white turbid secretion. This fluid was placed under the microscope, having previously been mixed with a little acetic acid. It contained no pus-globules. The glands in the groin were enlarged and indurated, but not inflamed.

July 27th.—The secretion from the sore was inoculated in several points on the patient's thigh.

29th.—The boy was admitted into the Lock Hospital. The inoculations had been followed by no result. The secretion from the sore was again examined, and found to contain no pus.

31st.—The sore, which continued to increase in size, had been dressed with linen and cold water since the last report. The linen appeared to have irritated the sore in some degree, and numerous globules now appeared in the secretion; but upon the addition of acetic acid, the distinct outlines of the

pus-nuclei were not visible. Several fresh inoculations were made. The sore was dressed with wet lint.

Aug. 3d.—None of the inoculations had succeeded. The secretion from the surface of the sore, placed under the microscope, and treated with acetic acid, did not appear to contain any pus. The sore was ordered to be dressed with blistering plaster. The glands at the back of the neck were now enlarged, and the skin presented for the first time the appearance of a syphilitic eruption.

5th.—None of the former inoculations had been followed by any effect. The application of the blistering plaster had produced a superficial slough on the surface of the sore, and blistered the surrounding skin. Some distinct pus-globules were now visible in the secretion from beneath the slough. This secretion was inoculated upon the thigh in several points.

7th.—The sore now again secreted no pus. Fresh inoculations were performed.

10th.—The sore was dressed twice yesterday with the sabine ointment, and it now yields a copious secretion of pus. This was inoculated in several points in a fresh place upon the thigh.

12th.—The inoculations last made had succeeded. The primary sore still yielded a copious secretion of pus.

14th.—The sore had been dressed with lint, kept wet with water, since the last report, and now no pus could be detected in the secretion.

The inoculations, both of the 5th and of the 10th, had now succeeded. They presented the appearance of circular red patches, with some elevation and thickening of the cuticle. In one place there was the appearance of a broken vesicle, from which a serous secretion exuded. This secretion from the inoculation was inoculated on the thigh.

17th.—The inoculation from the inoculation had succeeded. It presented the appearance of a red circular patch, from which the cuticle was abraded, with slight thickening of the skin. It had not at all assumed the appearance of a pustule, nor was anything like pus secreted from its surface. A single pustule, surrounded with very little inflammation, had formed in one of the points first inoculated. The eruption on the surface of the skin was fading. All the inoculations in this case were made with a lancet used for no other purpose, kept carefully wiped, and wrapped in paper.

19th.—The inoculations appeared as separate red patches on the skin, which in these situations was slightly raised and thickened, but no induration extended into its substance. The solitary pustule which had appeared had dried up. The original sore was healing, but being dressed with the sabine ointment, it afforded a purulent secretion, which was inoculated upon a patient who never had had constitutional syphilis, but who was apparently suffering from organic disease of the liver.

21st.—The inoculations presented the same appearances as before; they appeared covered in certain parts with thin scales; they were tender to the touch, with slight thickening on the surface, which did not extend into the substance of the cutis. The inoculations nowhere presented any appearance of ulceration. The inoculation performed on the 19th had produced a small, dark-brown, circular patch, in which the skin was slightly elevated. The secretion from the original sore was now again inoculated on a second female already affected with constitutional syphilis.

24th.—One of the inoculations on the boy has a slight tendency to ulcerate. The others are desquamating and losing their colour.

The single inoculation on the first woman unaffected with syphilis has assumed the form of a small red pimple. The redness gradually fades into

the colour of the surrounding skin. The cuticle at the inoculated part is thickened and elevated.

The inoculation on the second woman already affected with constitutional syphilis is less marked. There is only a slight redness and elevation at the inoculated part.

From the facts which have been brought under notice the following very important points are proved :

"1. That some primary syphilitic sores cannot readily be inoculated in the ordinary way, either during their period of progress or afterwards.

"2. That the sores which are not capable of being thus inoculated, as far as we have hitherto seen, are those affected with specific adhesive inflammation, and which do not, except under conditions of artificial irritation, secrete pus.

"3. That these sores, although not capable of being inoculated in the ordinary way, will nevertheless, when irritated, furnish a secretion which is capable of being inoculated.

"4. That the inoculations thus produced do not give rise, as a rule, to either suppuration or ulceration, but to some adhesive form of inflammation.

"The inoculations to which I have now directed your attention, and the results of which may be seen in two other patients now in the hospital, besides those to whom I have above referred, have all assumed a remarkably uniform character. There are many points of the greatest interest connected with this new form of artificial inoculation, which I hope to enter upon at length in one of the courses of lectures which will shortly be given at this hospital."

II. SPECIAL QUESTIONS IN MEDICINE.

(A) CONCERNING THE NERVOUS SYSTEM.

ART. 26.—*Hysteria, considered as a connecting link between mental and bodily disease.* By Dr. W. CAMPS.

(*British Med. Journal*, March 19, 1859.)

DR. CAMPS'S object is to draw attention to such forms of hysteria as present marked evidence of *psychical*, in addition to, or complication with, *somatic* affection. Until within a comparatively recent date, it has been the custom to consider mental disease as unconnected with or independent of bodily disease. But insanity is far more a bodily disease than has been hitherto considered; and in cases of this malady there is mostly, if not always, impairment of the proper healthy cerebral structure.

Dr. Camps gives a brief account of the hysterical paroxysm, which is almost, though not exclusively, confined to the female sex. It is, how-

ever, in the moral state and motives, which not unfrequently attend or accompany the severer forms of hysteria, that we may recognise without difficulty the approximation of this disease to some of the forms of mental disease. In severe cases of hysteria, amidst considerable general disorder, the nervous system being chiefly involved, the proper functions of the brain often become deeply affected. There is observed in such an increased susceptibility to impressions, a great rapidity of movements, together with a capriciousness of motives; the countenance indicates the alterations that take place in the feelings of the patient; and in the worst cases, the most amiable sentiments are observed to be converted into the most unamiable and repulsive. In some, there appears to be, at times, a complete metamorphosis of the whole moral character. This state of the disease becomes a subject of the deepest importance, more especially in regard to its moral treatment, so far as regards the imposition of personal restraint, or confinement of the patient; for nothing would be more injurious to a hysterical patient in this condition than undue interference with personal liberty. The functional activity of the spinal cord, as well as of the brain, may be morbidly diminished, or augmented, or perverted; and this latter state is especially the condition in hysteria. The perversion of the functional activity of the spinal cord is most marked in some cases, in which there is an extreme irritability of the cord at least, if not of the entire cerebro-spinal axis; and in the severer forms of the disease there doubtless exists an excessive irritability of the whole nervous centres; and it may be, of the entire nervous system, including even the nerves themselves. It might be assumed, that the irregular, convulsive, and impulsive actions of hysterical patients, depend rather upon some state of the blood, which alters its healthy relation to the nervous tissue, than upon any structural alteration of the proper nervous tissue itself.

During the last nine months, Dr. Camps has had under his care a somewhat severe and remarkable case of hysteria, which, he thinks, is one that afforded him good reasons to conclude that it, and similar cases, present phenomena that clearly show a connexion between mental and bodily disease. The subject of this affection was a lady, above fifty years of age, of a highly nervous temperament, who first came under his notice, presenting many of the ordinary characters of hysteria; but, as the disease gradually developed itself, this patient presented, in addition, many of the characters of a very protracted and aggravated case of this disease. There were paroxysms of choreic movements; at first, chiefly confined to the lower extremities, and to the left side of the trunk; these gradually affected, in a slighter degree, the upper extremities; being attended then with considerable paroxysmic palpitation of the heart, and heavy, laboured respiration. Then supervened excessive restlessness of the body generally, so that, when out of bed, the patient was almost always in bodily action, seldom or never sitting, frequently not even when at meals; in motion whilst standing, and very frequently walking hurriedly about in various apartments of the house. This was followed by, and accompanied with, excessive talkativeness, so as to fatigue her companions. When remonstrated with, and requested to be silent, her reply was, "I must talk, for I cannot help it." The subject of her conversation was almost invariably herself, and her peculiar ailment and

condition, and this ultimately assumed the form of intense selfishness or egotism. There was no derangement of the special senses, nor of the general cuticular sensation. The disease appeared to ascend gradually higher along the cerebro-spinal axis, and at length to reach the sensory ganglia, and the cerebrum itself; for certain *psychical* phenomena presented themselves at times, bearing the closest affinity to those exhibited in some forms of mental disease. Commonly, the patient slept well during the earlier part of the night, and seldom or never complained of pain in any part of the frame. The faculties of perception and memory were unimpaired, as was the judgment also, in relation to all matters of business demanding close attention; the imagination was in too lively exercise at times, but the will appeared to have lost its controlling power over the current of the thoughts. The patient would frequently remark, "My thoughts master me; I cannot help thinking about myself; I seem at times to have lost the power to think of anything, or of anybody, besides myself." Frequently, but not always, there was great irritability of temper; and this would sometimes be expressed in words, and sometimes in actions. The will having lost its controlling power, the impulses arising from this state of excessive emotional sensibility were occasionally expressed in very irregular and extraordinary phrases and actions of the body.

The view the author takes of this case was, that it is mainly connected with the cessation of the menstrual function. Moreover, in this case, there was excessive irritation, possibly congestion, of the greater part of the cerebro-spinal axis, including at least the sensory ganglia at the base of the brain—a condition of parts sufficient to account for most if not for all the symptoms connected with the nervous system.

Dr. Camps concludes by a brief reference to the treatment of hysteria. The measures recommended are: 1. The use of such agents as improve the general health, and especially the general state of nutrition of the nervous tissue; 2. Of such agents as remove the exciting causes of the paroxysms; and lastly, of all such agents as are likely to act beneficially upon the mental state and condition of the patient.

ART. 27.—*On irregularity of the Pupils from central causes.*

By Dr. RICHARZ, of Endernich.

(*Jour. of Mental Science, and Dublin Hospital Gaz.*, Feb. 1, 1859.)

Dr. Richarz's views were communicated to the Psychiatrial section of the Philosophical Association of Germany. They are translated from the 'Allgemeine z. f. Psychiatrie,' by Dr. E. Palmer.

"In describing the relative sizes of unequal pupils, in the diseases of the central organ of the nervous system (as in incomplete general paralysis), most observers make special mention of the dilated pupil; and, under precisely similar essential conditions we more frequently find one pupil characterised as being larger than the other, than the converse. Were there no prejudice at the bottom of this custom, there might be nothing to advance against it; but I believe that the views on which it is founded are more or less conjectural. It is apparently assumed, in the first place, that inequality of the pupils is always caused by lesion of one

iris only; that dilatation of the pupil is more truly and more frequently a morbid condition, than contraction; and finally, that dilatation is always dependent on relaxation resulting from paralysis. The iris with the dilated pupil is thus more often pointed out as being affected, and that with paralysis, than the one in which the pupil is contracted; and we find, moreover, that it is quite usual to adduce, not, perhaps, mere inequality, but dilatation of the pupils generally, as an absolute sign and example of paralysis of single muscles.

"These hypotheses—certainly not always clearly understood—are, however, completely false. It must be remembered, that both abnormal dilatation and abnormal contraction may each arise from two totally different conditions of iris—the dilatation either from paralysis of the circular or spasm of the radiated fibres; and the contraction, either from paralysis of the radial or spasm of the circular fibres; so that both irides may be affected, although, as a rule, only one of them is found to be so. Whether lesion is present in both or only in one, and in the latter case in which, can only be ascertained by careful observation. I have noticed, for example, that the difference in size between the pupils, in cerebral disease, does not usually remain the same under all conditions of light; also, that this difference will not probably be at its maximum under a moderate amount of illumination, but under the influence either of a very strong or a very feeble light; and that under the opposite extreme it falls to a minimum, or, indeed, disappears altogether. I speak, of course, only of relative difference, not of absolute, for the latter must naturally be less when the pupils are contracted, than when they are dilated. This, which is the usual demeanour of unequal pupils under various stimulus of light, can only be explained by regarding the lesion as being limited to one iris, and proves, indeed, that such is ordinarily the case. The skilful employment of rapid alternations of very strong and very feeble light, by which only the investigation can be made with exactitude, is hence a very proper and sound mode of diagnosis.

"From the application of this test the following possible cases result, including one, however, which for the most part is non-pathological.

"I. If, under varied amounts of illumination, the proportional difference between the pupils remains unaltered, while at the same time their absolute size varies in ratio to the strength of the light, both irides must be considered sound, and the inequality of the pupils regarded as arising from simple 'vitium;' or at least not as central, and not as depending on paralysis or spasm.

"II. If, under the influence of different degrees of light, the proportional difference between the pupils, and also their absolute size, remains wholly or nearly unaltered, both irides must be considered seriously and equally diseased—that with the larger pupil, either from paralysis of the circular, or spasm of the radial fibres; and that with the smaller pupil, either from paralysis of the radial or spasm of the circular fibres. Four possible cases occur here, from combination, viz.:

"1. Paralysis of the circular fibres in the larger pupil, with paralysis of the radial in the smaller (the most frequent).

"2. Paralysis of the circular fibres in the larger pupil, with spasm of the circular fibres in the smaller.

"3. Spasm of the radial fibres in the larger pupil, with paralysis of the radial fibres in the smaller.

"4. Spasm of the radial fibres in the larger pupil, with spasm of the circular fibres in the smaller.

"III. The last and most frequent occurrence is, that the inequality of the pupils nearly or wholly disappears either under a very powerful or very feeble impression of light. In this case only one iris is affected.

"1. If the inequality disappears under the influence of a powerful light, it can only be due to the contraction of the larger pupil; while, at the same time, the smaller pupil is not at all, or not equally, excited to contraction by the increased stimulus. We have, then, here the larger pupil pertaining to an iris, which is obedient to changes of light—or, in other words, sound; and the smaller, fixed, or imperfectly mobile pupil pertaining to an iris in a state of disease, from—

"(a) Paralysis of its radial fibres, by which the antagonistic circular fibres, which produce contraction of the pupil, obtain preponderance; or

"(b) Spasmodic contraction of its circular fibres.

"2. If the inequality of the pupils disappears, on the other hand, under a very feeble light, this can only be produced by dilatation of the smaller pupil; whilst, at the same time, the larger pupil remains uninfluenced by the diminution of light, or does not dilate in equal measure. Here the smaller pupil pertains to an iris obedient to light—*i. e.* healthy; and the larger fixed or imperfectly mobile pupil, to an iris in a condition of disease from—

"(a) Paralysed circular fibres, by which their antagonists, the radial fibres, obtain preponderance; or

"(b) Spasmodically contracted radial fibres.

"There are thus, in all, nine—or if the first (non-pathological) is omitted, eight—possible cases of inequality of the pupils. When the inequality continues with a certain constancy, the question of spasm can scarcely be entertained. In irides, however, which are equally and similarly effected, chronic spasmodic contraction is not unfrequently observed. The persistent dilatation of the pupils in helminthiasis cannot be explained as proceeding from paralysis, but as due to spasmodic contraction of the longitudinal fibres of the iris, from irritation of the sympathetic nerve. Inequality of the pupils, arising from spasm of the radial fibres of one iris, combined with spasm of the circular fibres of the other, is rare; but most rare is the occurrence of inequality through paralysis in one iris, with spasmodic contraction of the corresponding muscular fibres in the other.

"If, from the above eight possible conditions, all cases of a spasmodic character be withdrawn, as is mostly necessary in general paralysis, there will remain only three for consideration, viz.:

"1. Paralysis of the radial fibres of the iris with the smaller pupil.

"2. Paralysis of the circular fibres of the iris with the larger pupil.

"3. A combination of 1 and 2 with each other.

"The last of these is the least frequent in incomplete general paralysis. In this disease a single iris is much more often affected with paralysis than both; so that, independently of the general palsy, something of an hemiplegic character is usually present. I have further noticed, that when the irregularity disappears under one of the extremes of illumina-

tion, it more frequently takes place under the strongest than under the weakest light; so that, contrary to what is generally supposed, the defect is oftener in the contracted pupil than in the dilated one. This fully harmonises with the observation of Budge ('Movements of the Iris,' &c.). that the 'nerve oculomotorius,' which is known to be the excitor of the 'muscle sphincter iridis,' develops much more nervous force, and can be thrown into activity by a far less amount of stimulus, than the 'nerv. sympath.' which supplies the dilator muscle. It is therefore, *cæteris paribus*, far more likely, in general palsy, that paralysis occur in the nerves and muscular apparatus subserving the contraction of the pupil, than in those which effect its dilatation. In this regard the dilator and sphincter muscles of the iris hold the same relation to each other as the extensors and flexors of the limbs.

"Moreover, in inequality of the pupils resulting from central paralysis, the knowledge whether both irides are affected, or only one—whether, namely, the longitudinal fibres supplied by the 'sympathetic,' or the radial fibres supplied by the 'oculomotorius,' are paralysed—cannot but be of great importance in the diagnosis of the central seat of disease."

ART. 28.—*Case of Chronic Hydrocephalus tapped four times.* By Dr. WILKS and Mr. BRYANT, Assistant-Physician and Assistant-Surgeon to Guy's Hospital.

(*Medical Times and Gazette*, Dec. 11, 1858.)

CASE.—John Frederick R—, æt. 9 months, was admitted into Guy's Hospital, under the care of Dr. Wilks, June 20th, 1858. His father and mother are healthy; but his mother's father, two brothers and sister, all died of phthisis, and her nephew is the subject of hydrocephalus. His mother's labour with him was natural; and he was born a well-formed and apparently healthy child. When five weeks old he suffered from convulsions, which were occasionally repeated during the following fortnight. His mother, when he was three months old, first noticed that there was fulness of the anterior fontanelle, from which time she states that the head has continued to enlarge; and symptoms indicative of cephalic mischief, such as sudden screaming out at night, fingers clenched over his thumbs while asleep, and occasional attacks of convulsions, showed themselves. The head was ordered by Dr. Wilks (whom she attended first as an out-patient) to be strapped; this was done for the first time on the 30th March, 1858, at which time the head measured twenty-one and a quarter inches in circumference, and this was repeated four times; the child screaming at each time very much, and the mother, being tired of a process which did not prevent increase of the head, requested that something else might be done. While the strapping was being used the child's limbs and body became more developed and plump. Dr. Wilks, in conjunction with Mr. Bryant, determined to tap the head, which was done by the latter at 2 p.m. on the 29th June, 1858, by introducing a trocar and canula at the anterior fontanelle, a little to the left of the median line, after an elastic band had been bound round the cranium, more than five ounces of a transparent, colourless, slightly albuminous fluid of sp. gr. 1004 were allowed to flow, after which the opening was closed. The child did not suffer at all from the removal of the fluid, nor was the pulse, which was previous to the operation 150, affected. The child cried a great deal during the following night. On the

30th of June, 11 a.m., the trochar and canula were introduced at the same spot after the child had been placed under the influence of chloroform, and $\bar{3}v.$ $\bar{3}ij.$ of fluid, slightly tinged with blood, but free from flakes of lymph, were drawn off.

July 2d.— $\bar{3}ij.$ $\bar{3}ij.$ as clear as that first drawn were removed to-day. The child has been pretty well, not fretting so much as it did after the first operation; there is, however, some jerking of the limbs during sleep, and the thumbs are clenched over by the fingers.

4th.—It was tapped a fourth time to-day, and $\bar{3}iv.$ of clear fluid removed; the bandage was removed and reapplied, this being necessitated by the side of the head in the front of the left ear, which had been irritated by the strapping, threatening to ulcerate under the continued pressure of the bandage.

8th.—The part alluded to, as well as another spot on the forehead, has ulcerated, and presents an unhealthy appearance. The child seems pretty well, and is quiet.

12th.—Last night the child had an attack of convulsions; the ulcers are very unhealthy, being covered with a grayish slough; all bandaging has been done away with for several days past, and the water dressing is applied to the ulcers.

14th.—The mother left with her child to-day for the country, in order, if possible, to improve its health; and it was heard that the child died a fortnight afterwards. Although the paracentesis failed to afford any permanent relief, it was surprising to see with what impunity the brain bore the successive tapings.

ART. 29.—On "*Ataxie locomotrice progressive*."

By Dr. DUCHENNE, of Boulogne.

(*Archiv. Gén. de Méd.*, Jan., Feb., March, April, 1859.)

Progressive abolition of the power of co-ordinating movement, and apparent paralysis, contrasting with the integrity of the muscular force—such are the characteristics of the affection which Dr. Duchenne proposes to call "*ataxie locomotrice progressive*."

Individuals thus affected cannot preserve the erect position without lurching or falling; they cannot walk without support, and with support their legs are thrown about in a very disorderly manner. These disorderly movements, moreover, are most marked when the muscles are required to obey the mandates of the will. They are not accompanied by clonic spasms; they have no relation to chorea; they are evidently not caused by any fault in the muscular contractility, for this, as tested by electricity, is perfect, and the muscles themselves present no tendency to pass into a state of fatty degeneration; they are preceded or accompanied by various signs of disorder, mental and nervous; they get worse and worse, or better and better, in a longer or shorter time—a period varying from some months to several years. It is not easy, indeed, to seize the distinctive features of the affection; but the chief point appears to be, that there are some cases of paraplegia, and other forms of paralysis, in which the power of moving the muscles fails gradually, while the muscles do not lose their faculty of being moved—a

fact which we have not now to learn for the first time, though it is now more forcibly impressed upon our attention by the able investigations of Dr. Duchenne upon the electro-contractility of individual muscles.

ART. 30.—*The pathology and treatment of the Paralysis of Motion.*
By Dr. J. P. BATCHELDER.

(*American Medical Monthly*, Oct., 1858.)

Dr. Batchelder proposes to recover the lost power "by causing the members to perform the motions calculated to produce the changes in the brain. The motions first, and the changes in the cerebrum next, is the order and method of nature."

There are different conditions of the muscles in paralysis, which according to Dr. Batchelder require separate attention; some are powerless, some contracted, some acting irregularly, but generally speaking "all concur in disregarding the volitions of the patient."

From these conditions the following indications of cure are deduced:

1. To restore strength to the powerless muscles.
2. To overcome abnormal contraction in others.
3. To obviate or counteract the perverse action of such as act irregularly.
4. To renovate the will, and reconnect it with the affected muscles.

In the treatment proposed to meet the first two indications, Dr. Batchelder makes use of an "agent inherent in the muscles themselves, called by modern physiologists, 'passive contraction.' This power causes a muscle to contract and shorten whenever its origin and insertion have become approximated." Thus, by bringing the origin and insertion of the muscles as near as possible together, and retaining them in such a position by mechanical means until this "passive contraction" has taken place, a certain degree of power is restored to the paralysed limb. The apparatus is occasionally removed and the limb moved in different directions, the attention directed to these movements, the will taught to assist by concentration of effort upon them.

The perverted or irregular actions of muscles are also met by mechanical appliances until the one or the other yield to well directed efforts, which are more or less under the control of the will.

The first three indications point directly to the fourth, which is, in fact, the ultimate object aimed at, viz.: To renovate the will and reconnect it with the affected muscles.

This is effected by educating the will. The passive movements, the mind constantly directed towards them, act through the peripheral brain and gradually produce those cerebral changes which precede consciousness. This being frequently repeated, the will gradually awakens, regains power by degrees, and finally ends by producing those movements which are at first passively made.

ART. 31.—*On Chorea in reference to its connexion with Rheumatism.*

By Dr. T. P. HESLOP, Physician to Queen's Hospital, Birmingham.

(Dublin Quarterly Journal of Medicine, Nov., 1858.)

The cases related by Dr. Heslop in this paper, fourteen in number, furnish additional evidence that rheumatism has a very frequent clinical connexion with chorea, and that it may give both a predisposition and afford an exciting cause to the convulsive malady. A rapid summary of the cases in reference to the rheumatic condition is this :

The first case appears to be dependent exclusively upon an excited emotional state—grief for the loss of friends ; had a previous attack. The second gives us no information as to the cause ; had been attacked nine months before, and never been quite free from it since ; urine of high specific gravity. The third case is stated to have never had rheumatism ; and there is a total absence of the ordinary causes of the malady ; she is said to have had chest-pains during the attack. The fourth case supervened “shortly after” a general attack of rheumatism ; a loud cardiac bruit at the apex was noted ; while under observation, had rheumatic symptoms. The fifth case presented a loud bruit at the apex, and along the left side of the sternum ; the urine highly acid, of high specific gravity, was turbid, and deposited uric-acid crystals. The sixth case complicated pregnancy ; her mother is very subject to rheumatism ; the heart's action vehement, but without bruit ; urine, specific gravity, 1015. The seventh case appeared on the subsidence of an attack of rheumatism ; a slight cardiac bruit was noticed ; copious deposit of urine, specific gravity, 1035. The eighth case complicated pregnancy ; serious mitral disease and hypertrophy were found after death. The ninth case is the same as No. 1 ; it is the third attack ; pains in the back and chest ; a systolic bruit at the base of the heart and up the sternum, which persisted when movements had disappeared ; pain in the præcordial region ; specific gravity of urine 1020. The tenth case exhibits the third attack of the disorder ; complained of pain in joints, especially wrists and shoulders, and beneath the mammæ ; a loud systolic bruit audible an inch below the nipple. The eleventh case is the second attack of the disorder ; never had rheumatism, but a systolic bruit existed in the cardiac region, which was unaltered when discharged ; urine, specific gravity, 1020. The twelfth case is, probably, a fifth or sixth attack ; never had rheumatism ; notes of case imperfect. The thirteenth case is as much the history of rheumatic attacks as of chorea ; and while in hospital suffered from rheumatic pains in the joints ; the cardiac bruit was, probably, both organic and inorganic ; when discharged, it had almost disappeared. The fourteenth case was admitted, labouring under pericarditis, with chorea ; had never suffered from rheumatic pains ; but his father severely so. The fifteenth case appears to have no relation to rheumatism ; the heart is said to exhibit “slight irregularity.” The sixteenth offers no statement relative to rheumatism, but a mitral bruit was heard at the apex ; cerebral symptoms of unusual severity supervened ; record of dissection unreliable. The seventeenth case is a second attack ; suffered from palpitation of the heart ; but the record supplies no infor-

mation relative to rheumatism. The eighteenth is the same subject as the fifth; three years and a half have supervened between the attacks; nothing is stated about rheumatism; and it is note-worthy that the bruit formerly observed had entirely disappeared, as repeatedly mentioned in the report. The nineteenth case closely supervened upon an attack of acute rheumatism. Remarkable hysterical symptoms appeared during the progress of this case, coincident with a very striking change in the character of the urine. The indications of laryngismus and trachelismus were most formidable, and had relief not been quickly obtained, it would have become necessary to seek it in the operation of bronchotomy. The twentieth case offers, in a very young child, an interesting illustration of the relation of chorea to rheumatism; a marked attack of the latter had preceded the outbreak of the former. In a child three years old, it is difficult to imagine hysterical mobility to have much to do with the causation of the disorder.

(B) CONCERNING THE RESPIRATORY SYSTEM.

ART. 32.—*On the treatment of Croup by "tubage" of the Larynx.*

Dr. BOUCHUT, Physician to the Hôpital Ste. Eugénie, Paris.

(*L'Union Médicale*, Sept. 6, 1858.)

This mode of treatment consists in the dilatation of the larynx by means of "viroles" or ferrules, which are introduced into the larynx over a catheter, and allowed to remain there, when the symptoms of asphyxia become urgent. In performing the operation a curved male catheter, open at both ends, and of the proper size, is first introduced into the larynx. This serves as the guide for the "virole." The virole is a cylinder of silver, from one third to three fourths of an inch in length, and a little wider at its upper than its lower end. On its outside are two ridges, and the space between them is intended to receive the inferior vocal cord. When the "virole" is in position, indeed, it is completely within the larynx, and it offers no *mechanical* impediment to the free play of the epiglottis and the arytenoid cartilages. In introducing the "virole," a broad metallic ring is required to protect the forefinger from the teeth; for removing it a silk thread is attached, which is allowed to hang out of the mouth.

Dr. Bouchut proposes the treatment by "tubage" as a substitute for tracheotomy, which latter operation he regards as scarcely justifiable. In the long discussion, however, which was provoked in the Parisian Academy of Medicine—a discussion in which many of the ablest physicians and surgeons in the French metropolis took part—it was shown, not only that the statistics of tracheotomy in croup were by no means so unfavorable as they are represented to be by Dr. Bouchut, but that the results of the treatment by "tubage" were by no means encouraging. It was shown, indeed, that "tubage" had been unsuccessful in all the seven cases in which it had been then tried, and that in four of these cases (in one with success) it had to be followed by tracheotomy. The report of the Academy, moreover, which was drawn up by Dr. Trousseau, is con-

demnatory. It runs thus: 1. That "tubage" is of considerable difficulty of execution, and is a dangerous proceeding if the canula be left more than forty-eight hours in contact with the cordæ vocales. 2. That it is not impossible that this procedure may render some service in certain acute or chronic diseases of the larynx; but the facts thus far published are insufficient to demonstrate its utility in croup. 3. That to the present time, tracheotomy remains the sole measure applicable in croup, when all the medicinal resources have become exhausted.

ART. 33.—*On Croup.* By Dr. LUZINSKY.

(*J. für Kinderkrankheiten*, Nos. 9, 10, 1857, and *Edinburgh Medical Journal*, April, 1859.)

Dr. Luzinsky, director of the Mariahilf Children's Hospital, Vienna, in a long communication on croup, recommends the following treatment:—Every child with a hoarse voice and the peculiar cough, and some fever, must be at once put to bed, and gentle perspiration induced. If now, the voice becomes clearer, the cough loose, and the nose run, the danger is over. Should, however, the unfavorable symptoms continue or increase, and the breathing become more difficult, then more energetic means must be used. 1. To alter the blood crasis. 2. To prevent the localization of the inflammation in the larynx. 3. To prevent laryngeal spasm. 4. To destroy the membranous exudation, and promote its escape. The first indication is fulfilled by the alkaline carbonates, which dissolve albuminous and fibrinous products. Luzinsky gives daily from $\frac{1}{2}$ to 2 drachms of carbonate of potass, dissolved in 2 ounces of water and sweetened with half an ounce of syrup, till the cough becomes soft and loose. The second indication is best fulfilled by the energetic local application of cold. The whole body is well covered, and kept warm and dry; the neck is bared, and assiduously fomented with ice-cold water, the patient getting at the same time a constant supply of ice-cold milk and water in small quantities; this treatment is continued from one to three days, till the inflammatory symptoms begin to yield, when the cold is gradually left off and more nourishment given. Luzinsky objects entirely to local bloodletting, and where the application of cold is objected to, is in the habit of replacing it by a blister, the size of half-a-crown, on the manubrium of the sternum, which is to be kept open by epispasties. The third indication Luzinsky attempts to fulfil by the free exhibition of opium, acet. of morphia, $\frac{1}{8}$ — $\frac{1}{4}$ gr. in two ounces of water, and half an ounce of syrup to be given at longer or shorter intervals, according to the amount of dysprœa, cough, and restlessness present. To fulfil the fourth indication, Luzinsky sponges the fauces and larynx several times a-day with a solution of nitrate of silver, 4—8 grs. in half an ounce of water. When the pseudo-membrane loosens, and the larynx gets filled with tough mucus impeding the respiration, Luzinsky gives emetics, sulphate of copper 4—8 grs., in Zijss. sweetened water, a dessert-spoonful every half or quarter hour. Luzinsky is opposed to tracheotomy, but rather inclined to catheterise the trachea. Among 38,000 patients, Luzinsky has had 90 cases of croup (55 boys, 35 girls,) of whom there were—

Under 1 year old	11	4 to 5 years old,	9
1 to 2 —	16	5 to 6 —	15
2 to 3 —	16	6 to 7 —	14
3 to 4 —	8	9 —	1

Of these 90 cases only 15 died. Luzsinsky groups them in three divisions. The first—characterised by persistent rough voice, short, raw, barking cough, difficult breathing, with occasional sibilant râle, and more or less fever—were 36 in number, and all recovered. The second division comprehended all cases marked by a weak, thin, shrill voice, hollow tubular cough, laborious respiration, feeble and often hissing respiratory murmur, and great restlessness; of such there were 43, of whom 34 recovered, and only 9 died. In the third group the voice was reduced to a faint whisper, the cough choked and scarce audible, and there was great orthopnoea; of such there were 11, of whom 6 died. The result speaks favorably for the practice.

ART. 34.—*Glycerine as a local application in Croup.*

By Dr. E. R. MAYER.

(*Amer. Jour. of Med. Science*, April, 1853.)

In this paper Dr. Mayer refers to two severe and well-marked cases of croup occurring within a few days of each other. The one in a boy eighteen months old, the other in a girl four years old. In both the disease was developed during the decline of the eruption of measles. Suffocation seemed imminent in both cases. The treatment consisted in prompt emesis with sulphate of copper, by which the severity of the symptoms was mitigated, and afterwards, in the rapid introduction of calomel into the system; the use of from one to three grains of sulphate of copper, dissolved in water, every two or three hours, alternated at the same intervals with tartar emetic in nauseating doses, or the still more effective sedative, the veratrum viride, in doses of from two to four drops of the fluid extract; small blister over the upper part of the sternum, and the application of pure glycerine to the glottis, whenever its use was indicated by a return or increase of the brassy, ringing cough, or crowing respiration. The application of the glycerine was in each instance soon followed by a manifest softening of the sound of the breathing and cough, and a considerable reduction of the dyspnoea and general distress. This improvement usually lasted for a few hours, but by applying the glycerine at short intervals Dr. Mayer found that he was able to afford the patient an amount of permanent relief throughout the disease. In the younger of his patients expectoration of softened false membrane in shreds and patches, "a genuine *detritus*, mixed with altered mucus and a little dark blood," took place twenty hours after the application of glycerine was commenced, and in the other patient on the fifth day of the attack. "The croupy symptoms did not entirely disappear in either case until a day or two after the expulsion of the false membranes, but the patients were soon convalescent. Expectoration was promoted and recovery hastened by the administration of small doses of quinine and of chlorate of potassa with syrup of senega."

Dr. Mayer applies the glycerine by pressing down and drawing forward the tongue with the finger, and squeezing the contents with a long, thick camel's-hair brush, dipped in the liquid, over, as near as possible, the chink of the glottis, and then swabbing the whole throat. A sponge or portion of charpie may no doubt be preferable in some cases to the brush.

ART. 35.—*On Rheumatism of the Diaphragm.* By Dr. CHENEVIER.

(*Gaz. des Hôpitaux*, No. 35, 1858.)

After reporting several cases of this disease, the author gives the following description of it: The disease commences with a sudden pain at the points of attachment of the diaphragm, which produces a feeling of constriction at the base of the thorax, but is not augmented on pressure. Deep inspirations are impossible, and respiration is carried on only by the superior ribs. Percussion is normal, and auscultation does not reveal any change in the respiratory murmur, which is only somewhat weaker at the base of the thorax; there is no cough; sometimes, however, a painful hiccough. The abdominal organs offer no symptom of disease. The attack lasts from one to eight hours, and disappears then without leaving any trace. The prognosis is favorable. Rheumatism of the diaphragm is easily distinguished from inflammatory diseases of the lungs by the absence of the symptoms of the latter. It could only be mistaken for a neuralgic affection of neighbouring organs, as, for instance, intercostal neuralgia; but it is sufficiently distinguished from it by the pain being felt particularly in the three characteristic points, while in the neuralgia just mentioned it is confined to one side. From angina pectoris it is distinguished by the peculiarity that the pain proceeds in this malady from the sternum and radiates on one side to the arm. In nervous asthma, which also commences with sudden difficulty of breathing, the peculiar feeling of constriction as well as the confinement of the respiratory movements to the superior ribs, is not noticed; the two latter symptoms are pathognomonic of rheumatism of the diaphragm.

The treatment of the disease consists in the application of cups, mustard poultices, anodyne embrocations, and chloroform; if it is obstinate, the endermatic application of morphia will be useful.

ART. 36.—*On the use of small doses of Morphia in Hooping-cough.*

By Dr. C. M. MÜLLER, of Berlin.

(*J. für Kinderkrankh.* Nov. and Dec., 1857; and *Dublin Quarterly Journal of Medicine*, Feb., 1859.)

"The idea of employing morphia in the hooping-cough of children was suggested to me by the perusal of an essay published in the 'Medico-chirurgical Transactions' for 1854, by Edward Smith, of London. In this paper the author endeavoured to show that when death occurs in hooping-cough, the fatal result is usually due to bronchitis, and is not produced in the mode in which it ordinarily takes place in the so-called zymotic or acute diseases, depending on a poisoned state of the blood,

scarlatina, measles, smallpox, &c. It is possible that hooping-cough may have a certain degree of affinity with these diseases. Many circumstances favour such a view, particularly the facts that it usually occurs only once in life; follows a definite typical course, divided into stages of increase and decline; and, finally, the examples of communication by contagion which have been brought forward. The latter may, however, be deceptive, since, as is well known, under our social circumstances, the occurrence of contagion, for which direct contact is not absolutely necessary, cannot easily be proved with certainty; and, on the other hand, we are aware of the absence of all so-called crises, or critical excretions, which occur so decidedly and characteristically in scarlatina, measles, and small-pox, particularly through the skin and kidneys, and also through the mucous and serous membranes. But to establish a connexion between hooping-cough and these diseases, we should be able to prove intoxication of the blood. Such proofs, are, however, wholly wanting. We see no particular affection of the skin, no specific derangement of the kidneys, no indication of cachexy or dyscrasia. What we do see is pre-eminently a peculiar nervous affection connected with bronchitis; and did I venture on a systematic nosology, I should be rather inclined to place hooping-cough either among intermittent fevers, or in the great class of neuroses to which epilepsy, chorea, &c., are referred. In the treatment of hooping-cough we must bear the nervous element prominently in mind. To this we are compelled, whatever theory we may hold in respect to the disease. If we succeed in removing that which distinguishes pertussis from an ordinary bronchitic cough, or, in other words, if we succeed in changing the affection into a common catarrhal cough, we are content. We consider then that our work is done; and yet what is removed is nothing more than the purely nervous element. Experience has shown that all the influences which are capable of stimulating and increasing the renal function act favorably on hooping-cough—as fresh air, a regulated diet, attention to the bowels, &c. Having in the latter point of view duly acted on the liver and intestinal canal, we must now endeavour to exercise a direct influence on the peculiar affection of the nerves, which presents itself in hooping-cough. Dr. Smith, whose essay I have mentioned, is quite of the same opinion, and, as I have done, has directed his attention, not to belladonna or hemlock, aconite or henbane, but to morphia. I consider that none of these narcotic remedies is so much to be relied on as morphia; and belladonna, moreover, to be dangerous. ‘I endeavoured,’ says Dr. Smith, ‘to ascertain in what doses morphia may be given to children without affecting the head, and how far the dose may be continued. In these investigations I did not consider it advisable to proceed too slowly and cautiously, and I therefore increased the dose steadily until I had attained the maximum. If I had begun with the sixty-fourth part of a grain, as I should do with a child of four months old, I would by degrees give an additional dose, and at last administer three or four doses together every four hours; and if no drowsiness came on, I would augment the dose to one forty-eighth part of a grain, and go even still further, until a slight degree of narcotism should be produced. But if the slightest degree of narcotism or drowsiness occurred, I would, for two days or less, adhere to the dose I had attained to. Under this treatment the spasmodic hoop gives way, and

after three to ten days' use of the morphia, will have entirely disappeared, while the cough will have assumed an ordinary character. In many cases I found an evident improvement even on the second day, and on the fourth the hooping-cough had quite lost its specific character. I do not consider it necessary that the morphia should always be given so far as to affect the head, but the dose must be at least once increased to that point, because only in this mode can the measure of the influence of the remedy be obtained.'

"I have in my own practice obtained precisely similar results. I have commenced in very young children with the sixtieth part of a grain of morphia, and have increased the dose to the fortieth, or even to the thirty-sixth part of a grain, until a slight degree of narcotism or drowsiness has been manifested, and I have then persevered with the same dose until the hoop has quite ceased. My experience of the remedy is such as to enable me conscientiously to recommend this mode of treatment to my colleagues; and what Dr. Smith says on the subject is likewise very encouraging:

"'In hospital and private practice,' he says, 'I have repeatedly used morphia in hooping-cough, and always with a satisfactory result.' It is, however, necessary, as Dr. Smith also points out, that the diet and state of bowels of the children should be strictly attended to. It will be necessary to combine with the morphia treatment, according to circumstances, the use of aperients or of tonics. Attention must also be paid to the due ventilation of the rooms in which the children are kept."

ART. 37.—*On the treatment of Hooping-cough.*

By Dr. JAMES WHITEHEAD.

(*Third Report of the Clinical Hospital, Manchester*, Churchill, pp. 117, 1859.)

"Notwithstanding the notion, extensively prevalent, that hooping-cough is uncontrollable by remedies, or that it can be benefited by change of climate, there is," says Dr. Whitehead, in the very excellent report from which these remarks are taken, "no reason to doubt that, if brought early under treatment, the symptoms may not only be moderated, and other contingent diseases warded off, but its duration may be materially shortened. Enough has been already said on the subject to substantiate this assertion. The 35 cases brought under treatment, after an average duration of more than three months, were all cured in less than twenty-five days in the aggregate, and would, doubtless, have experienced the same beneficial result, and in about the same length of time, had they been brought six or eight weeks earlier.

"This assertion is further borne out by this fact, that of 87 cases brought for treatment within fourteen days of their commencement, the time occupied by the treatment was still the same as that of the general average, but the whole duration of the complaint was only thirty-seven days, that of the whole number, excluding the neglected cases, being forty-two days, and the term of the decidedly neglected cases, 111 days.

"Further, of the 87 cases above-named, 32 had an average existence

of eleven days, in which the whole term of the complaint was thirty-five days; and, of 55 cases, with an average existence of five days on admission, the whole term was reduced to thirty-two days.

"The remedies employed were, in the simple cases, or when the complicated cases had been reduced by other treatment to this condition, Dover's powder, alone, or combined with camphor, camphor inhalations, emetics, belladonna, and local irritants; but always with either opium (Dover's powder) or belladonna as a principal remedy. Sometimes the Dover's powder was replaced by tincture of opium, given in camphor or other aromatic water. The general modes were thus reduced to the *opium treatment* and the *belladonna treatment*, the results of which are as follows:

"Opium was commonly given in form of Dover's powder, in doses of one grain (containing one tenth of a grain of pure opium), or one or two drops of the tincture in aromatic water, twice or thrice daily. Frequently, the Dover's powder was combined with an equal quantity of camphor, and sometimes with half or a quarter of a grain of calomel, twice or three times a day, for a child twelve months old. This mode of treatment had an excellent effect in many cases. An equally successful result was often obtained by an emetic (5 grains or more of Ipecacuan powder), given in the morning, and 2 grains of Dover's powder, with or without camphor, at bed-time; no other medicines in the interim. By these measures, 58 cases were treated and cured on the average in 28 days.

"Belladonna was used in 76 cases. It was given in form of powder of the leaves, never the extract, as this is an uncertain preparation; and sometimes in form of solution of the nitrate of atropia. When in the form of powder, half a grain, mixed with five grains of sugar, was given to a child twelve months old, twice a-day; then, after two days, if well tolerated, three times, then four times a-day or oftener, and in larger doses, being gradually increased until a specific effect was produced. The solution of nitrate of atropia was prepared so as to contain one-ninety-sixth of a grain in a teaspoonful of the liquid; this dose of the salt is equal in its therapeutical effect to about half a grain of the powdered leaf, so that a teaspoonful of it may be given twice or thrice daily to a child twelve months old.

"The specific effect alluded to, called *atropism*, consists in an assemblage of phenomena which the system displays when charged with the remedy to a certain degree of saturation, analogous to iodism, ptyalism, or quininism. These symptoms are: dryness of tongue and fauces, with thirst; slight dyspnœa; redness, and sometimes puffiness of the skin of the features, neck, and chest; occasionally, but not always, dilation of the pupils; and now and then slight giddiness. It is not necessary, in all cases, to push the remedy to this pitch in order to obtain a curative effect; but in those who bear the remedy well, and in whom atropism is speedily induced, the disease, even in its severest form, and although in the stage of increase, is at once arrested, and, with due precaution, does not relapse. Thus, in several instances, brought early under treatment, in which atropism was brought about in the space of a few days, the duration of the attack was reduced to twelve, sixteen, or twenty days.

"Of the 76 cases treated by belladonna, 9 were very irregular in attendance, the treatment often being interrupted for a week or ten days at a time. In the other 67 cases, in some of which the attendance was also irregular, the average duration of the treatment was 22 days, giving a decided preference to this remedy.

"It is highly probable that were the belladonna treatment early adopted in each case, and associated with suitable hygienic regulations, the duration of the disease might be reduced from its average of 42 days to that of 28 or 30 days, and both its concomitant and consecutive accompaniments be materially lessened.

"The tolerance of belladonna is different in different subjects, and is probably as great in the young child as the adult. While a few half- or quarter-grain doses will suffice to atropise one, another will bear it for a length of time, in high doses, if augmented gradually. In a child four and a half months old (case 170), on the fourteenth day of the attack, a quarter of a grain was followed by alarming atropism. On the next day, the symptoms having subsided, and the hooping being relieved, another such dose was given, and followed by symptoms still more violent than the first. Further trials were not made. In contrast with this, in case 52, a child two and a half years old, the dose was increased from half a grain, twice, to six grains, five times a day—thirty grains daily, before a crisis was brought about.

"The value of some other modes of treatment will in future be tested.

"The diet of a patient labouring under hooping-cough should be carefully regulated. An error in this way is quite enough to aggravate or prolong the disease, or to cause a relapse after it has been absent many days. The aliment, whether animal, farinaceous, or vegetable, should be in the liquid or semi-liquid form, and such as is easily assimilated. The alimentary mucous membrane being in a highly irritable condition, the presence of solid food can with difficulty be tolerated, and often occasions great disturbance. A meal of solid food will often aggravate the paroxysms both in severity and frequency, and may induce a relapse after a cessation of several days or weeks. By a similar kind of sympathetic irritability the presence of worms in the intestines will aggravate the symptoms or prolong the duration of hooping-cough almost indefinitely; in several instances in which the symptoms continued unabated unduly long, and where it was found that worms existed, the expulsion of these parasites was immediately followed by mitigation of the paroxysms, and speedy cure."

ART. 38.—*Rules for the Dietetic Treatment of Asthma.*

By Dr. HYDE SALTER, Assistant-Physician to Charing Cross Hospital.

(*Lancet*, Nov. 6, 1858.)

1. The tendency of food to produce asthma is greatly increased by the state of sleep; therefore, nothing should be taken after such a time as digestion and absorption may be completely over in—the stomach and small intestines, and even the lacteals, quite empty—before bedtime.

2. This long fast before sleep involves a long period of inanition; therefore the asthmatic should break his fast early and heartily.

3. The quantity of food the asthmatic takes should be small; therefore it should be highly nutritious.

4. As a rule, the tendency of food to produce asthma is in direct proportion to its general indigestibility; therefore the asthmatic's diet should be of the simplest and plainest kind.

5. But there are some articles of diet that have a special tendency to produce asthma; therefore from these the asthmatic should exercise the strictest abstention.

ART. 39.—*On the Prognosis of Phthisis.* By Dr. E. LATHAM ORMEROD,
Physician to the Sussex County Hospital.

(*British Med. Jour.*, Dec. 25, 1858.)

"I believe," says Dr. Ormerod, "that there are two forms under which tubercle may be deposited in the lungs, two anatomical types of pulmonary phthisis, according as the disease is limited to the apex or diffused more generally throughout the lung. And the prognosis is more favorable, *cæteris paribus*, according as the case approaches nearer to the first of these types. The prognosis, however, does not rest exclusively on the physical signs; separately, indeed, the constitutional symptoms have much more value than the local signs in the question of prognosis. But the two classes of signs taken together enhance each other's importance, and possess a value collectively which neither can separately lay claim to. Thus—to suppose extreme cases—a great degree of constitutional disturbance, with a hurried pulse and a loaded state of the urine, is of the most unfavorable import, though the local changes be seemingly very slight, more especially if they be diffused over a wide extent of the lungs. On the other hand, slight constitutional disturbance may justify us in entertaining a more hopeful view of a case, though the local signs point to an advanced stage of the disease, if these changes be limited to the apex.

"Sharing fully in the abstract belief of the curability of phthisis, I share equally in the conviction of the very delicate nature of the evidence on which that belief rests. The more perfect the separation, the more completely is the proof of the previous existence of the disease removed. The more evident the physical or other signs, the less likely is reparation to ensue at all. It is only in the doubtful cases that there is much room for hope as the general rule; only in very exceptional cases is this hope realised.

"Speaking from the clinical observation of phthisis, however quiet and tractable the disease may seem up to a certain point, still we always find that some obstacle prevents our quite removing the disease. It is betrayed, may be, by none but curable symptoms. But we can get no further than the cure of these symptoms. And, without constant care, the disease will put on the ordinary character of phthisis, and other symptoms will develop themselves which we cannot cure.

"If, resting on anatomical distinctions, we point to cavities where

ulceration has been superseded by reparative action, or to harmless calcareous concretions replacing tubercular nodules, the difficulty meets us still. The very grounds on which we are arguing, show the incompleteness of the repair of the disease. The details I have given are but samples of many others, which each one from his own experience might supply, showing how far these conditions are from a cure. Nor can such a result, in the lowest practical sense of the word, be obtained even by the sacrifice of the organ affected, by collapse of the lung. We may set the tubercle at rest, perhaps. We may even, under favorable circumstances, reduce the injured parts to a condition differing little from the results of ordinary disease. But the most gratifying results obtained in this way would fall very far short of perfect success. For even to heal the mischief already done to the lungs, could we do this much, were not enough to constitute a real cure in the majority of cases. For the pulmonary disease, in the majority of cases, is but a part of phthisis."

ART. 40.—*On the management of the Shoulders in examinations of the Chest.*
By Dr. CORSON, Physician to the New York and Eastern Dispensary.

(*New York Jour. of Medicine*, March, 1859.)

The following is a summary of an elaborate paper in which Dr. Corson endeavours to show that by a proper management of the shoulders of the patient we may add fully one third to our power of detecting the earliest signs of consumption.

1. That remembering the great value of many reputed "little things," in the science of saving life; and that the chests of *lean* persons give clearest sounds, and are best marked—we may seize this hint from nature, and increase the "physical signs," by either lessening or removing more especially those principal natural obstacles, the great pectorals in front, and the two scapulæ and their muscles behind.

2. This may be affected by using the arms as levers, and the hands as hooks to pull. The process, in each case, involves three principles—*thinning, condensing, and tightening*. It is illustrated by the simple experiment of placing one forearm of a muscular man behind his back, while the other hangs loosely by his side, when the sound, especially of percussion, will be found heightened below the clavicle of the stretched side in front.

3. That the suggestions here offered are not fanciful theories, but the results of practical observations on several hundred patients in private, and in two large dispensaries, during the past year. To throw back the shoulders, and bare the whole front, we need the "*first position*." It is a repetition of the above experiment with *both* arms. *The left wrist is simply held easily with the right hand behind the loins*. This has many little advantages in obscure cases. It gives symmetry, gets rid of the arms, and fits the coat of flesh closely, like a bandage, for "inspection," makes it tense to increase the resonance of delicate percussion, and *conducts* better the sounds within. It thus aids in distinguishing the more difficult cases of tubercles, pleurisy, pneumonia, or aneurism.

4. That the "second position" is the common one of locking the hands

over the head to examine the axillæ, and is mentioned to avoid omission. The *third position* crosses the arms at the back of the head, with the hands grasping near the elbows, so as to *hoist* the shoulder-blades high up behind, and *thin* the muscles, to search for obscure or limited pleurisy or pneumonia low down near the diaphragm posteriorly.

5. It is very important early in suspicious cases of cough, to examine carefully the *tops of the lungs behind*. For without any distinct signs in front, consumption, often thus mistaken for a mere throat-affection, begins here. A few scattered tubercles are apt to *burrow*, as it were, beneath the top of the shoulder. Here we need the "*fourth position*." For this the patient crosses arms in front, slightly stooping, *hooks* the hands at the loins, or false ribs, and then stretching upward, he holds fast to increase the tension. The physician aids from behind, by pressing down firmly the shoulders. They are thus *slid off*, the muscles are smoothed down, and the ear, coming closer upon the top of the lung, hears better the sounds.

6. *As worth more than all the rest*, we commend the "*fifth position*," for by natural machinery it wrenches the shoulders forward *out of their beds*, and widely severs them in the rear. In thin persons it often thus stretches out their intervening muscles till, like stout broadcloth, it thus quite uncovers the inner and upper part of the lungs behind. To accomplish this, the patient crosses arms in front, with the stronger outside, grasps with the opposite hands the *two shoulder-joints*, pulls both strongly, and holds fast, to keep them tense. The physician aids to fix the shoulder-blades widely apart at the back by firmly pushing. Even in health, as any one can prove, the soft breathing murmur at the former place of the scapula can be thus nearly doubled. In tubercles it here opens a new field for *palpation*, and especially for *percussion*. It intensifies harsh respiration, or "fatty crackling." In pneumonia, it exaggerates the clear, barrel-like echo of "bronchophony," and in pleurisy that line between wind and water, the trembling "egophony." It brings out a delicate *new sign*, we have discovered, in bronchitis. It is a kind of prolonged liquid breathing, as if through a layer of wet sponge, heard before or after mucous rales, which we venture to name *moist respiration*.

7. Another new and really useful "physical sign" we have to communicate, is the *comparative stiffness of the shoulder over the lung most diseased, in strong breathing, seen and felt from behind*. For this we may use the "*sixth position*." Facing the back of patient, a yard distant, near a window or white wall, you tell him to drop his arms, let them hang easily by the sides, "as if dead," and then breathe deeply for a few moments, "like a man a little out of breath." You now "take aim," like a rifleman, across the top of the shoulders, and then shut your eyes and *feel* them gently swell. Drawing nearer, you notice that the "inferior angles" of the scapulæ move gently in breathing like the fins of a fish. You can both *see* and *feel* this movement. This *stiffness* of the shoulder in breathing may be decided or slight, local or general. When most at the top, we term it for convenience "acromial," and when most at the lower extremity, or inferior angle, we call it "angular." Curiously enough, these last features seem to depend on the higher or lower location of the disease which thus, as it were, *paralyses the parts nearest*. An elegant way of testing "angular stiffness," even in a lady fully clad, is

to place your two index fingers on the lower points of her shoulder-blades, and watch and feel their movement as she sighs. The causes of this stiffness are supposed to be *loss of upward expansion* in the lung, tenderness, pleuritic adhesions, and weight of morbid deposits. A table of eighteen cases is added, illustrative of this sign. It was least in recent attacks; varied most in phthisis; was slightest in pneumonia, and greatest in chronic pleurisy.

8. A statement of measurements of ten males, shows the gain in inches, and decimals, by "third," "fourth," or "fifth," positions respectively, between the inferior angles of the scapula and the lowest lumbar vertebra; the "superior angles" and the vertebra prominens of the neck and between the two upper and two lower angles of the scapula. Of the whole of the six positions, the first, fourth, *fifth*, and sixth are the most frequently useful. The others apply to particular cases. Taking into account the pulmonary complications of other diseases as well as the range of "chest-disease," it is believed these various improvements, slight as they seem in detail, really throw light, perhaps, upon many forms of *one third of the fatal maladies of the race*.

9. On account of its fearful importance, it is hoped they will mainly benefit *tubercular consumption*. Tracing faithfully, by various "marks," and the unhealthy habits of the patient, the agencies leading to the two prevailing causes, *feeble organization and depraved nutrition*—by prompt reform of abuses, generous *animal food*, and free exercise in the *open air*, with tonics, and *cod-liver oil*—we may do much to arrest the disease. *Occasionally, we may cure*. The encouraging researches of Hughes Bennett and Messrs. Rogée and Boudet show that from the numerous chalky concretions, puckerings, and cicatrices found at the tops of the lungs in very aged persons, it is probable that *about one half have recovered* from more or less tubercular deposits during their lives. Four living cases from several others are reported by the writer of arrest or cure of phthisis of several years' standing. The great question of this paper then is, *What may be the result of average notice, say three months sooner?* Time only can tell. Each physician who reads this is earnestly requested to aid by a faithful trial of this system of examinations in at least *three suitable cases*. The malady is still widely and deplorably fatal. From extensive trial, we firmly believe that, simple as they may seem, this *management of the shoulders*, these expedients for *thinning, condensing, and tightening the fleshy walls of the chest*, add *fully one third to our power of detecting the earliest signs of consumption*.

ART. 41.—*On interrupted or Jerking Respiration.*

By Dr. A. BOURGADE.

(*Archiv. Gén. de Méd.*, Nov. 1858.)

Dr. A. Bourgade attaches much importance to interrupted or jerking respiration (respiration saccadée) as an early sign of pulmonary tubercle, and he enforces his opinion by the narration of nine cases in which this sign preceded all others. Interrupted or jerking respiration, he tells us, is heard principally during inspiration; and, as a rule, it ceases to be audible when the expiratory murmur has become distinctly prolonged, or

when other unmistakeable signs of advancing disease have made their appearance. Under any circumstances, it varies much in force and distinctness, and in some instances it may only make its appearance occasionally at intervals of greater or less length.

(C) CONCERNING THE CIRCULATORY SYSTEM.

ART. 42.—*Pericarditis caused by a foreign body in the Œsophagus.*
By Dr. BUIST.

(*Charlestown Med. Jour. and Rev.*, Jan., 1858.)

CASE.—The patient in this case swallowed two artificial teeth and the plate belonging to them. On the day following he complained of sharp pain in the epigastrium, but only for a short time. Five days later the epigastric pain returned, and along with this there was much fever and obstinate vomiting. The day following there was delirium and evident signs of sinking. Death happened shortly afterwards. In the autopsy, the stomach was a good deal injected in the neighbourhood of the pylorus, and the pericardium was inflamed and its cavity filled with fetid gas, and a greenish sero-purulent liquid. The artificial teeth and their plate had stuck in the œsophagus a little above the cardiac opening of the stomach, and it had made a distinct perforation through the anterior coats of the œsophagus and the posterior aspect of the pericardium.

ART. 43.—*Treatment of Acute Anæmia.* By Dr. JAUZER.

(*Prager Vierteljahrsch.*, Bd. 4, 1859, and *Edinburgh Medical Journal*, April, 1859.)

According to Dr. Jauzer, the immediate danger of death from sudden loss of blood may be averted by artificially restricting the circulation to the trunk and head, laying the body horizontal, and elevating the limbs and compressing the femoral artery against the ileo-pectineal tubercle, and surrounding the limbs with a firm bandage. The chief difficulty lies in the length of time during which the compression may be necessary ($\frac{1}{2}$ —12 hours).

ART. 44.—*On the occurrence of a blowing sound in the Pulmonary Artery.*
By Dr. J. DA COSTA, Lecturer on the Practice of Medicine in the Philadelphia Medical Association.

(*Amer. Journ. Med. Sciences*, Jan., 1859.)

In examinations of the chest, Dr. Da Costa has met with a blowing sound in the second intercostal space on the left side, the occurrence of which has only been imperfectly attended to. This sound is heard in the exact course of the pulmonary artery, and especially at a point corresponding to the bifurcation of its left branch; and there is every reason to believe that it must be referred to this vessel. Dr. Latham, long ago, pointed out the occurrence of a blowing sound at the upper part of

the left lung in persons who were undeniably tubercular, or suspected of being so, and Dr. Da Costa's object is chiefly to recal attention to the forgotten observation, to explain its cause. Dr. Da Costa relates eight cases, in all of which local signs of partial consolidation were met with, which consolidation, he thinks, might prevent the vessel from fully and equally distending, and thus give rise to a murmur; and this explanation has every appearance of probability, for in no one of the cases could the murmur be referred to anæmia, or to the presence of local obstruction in the vessel, nor could it be supposed to be transmitted from the heart.

"The physical sign in question," says Dr. Da Costa, in his description of it, "may be stated as being a murmur attending the impulse of the heart, almost always soft and low-pitched, although occasionally harsher, of higher pitch, and simulating a sibilant rôle. Its situation is in the second intercostal space on the left side, not an inch from the edge of the sternum. It may be audible higher up, or opposite the third rib, on the sternum.

"All blowing sounds in the heart or in arteries are due either to a peculiar condition of the blood, or to the presence of local obstructions; or, again, they may be transmitted. This latter supposition cannot be entertained, as there was no murmur anywhere to be transmitted, the heart in all the eight cases yielding perfectly normal sounds.

"The space it occupies is usually very limited, and can be accurately circumscribed with the stethoscope. It is not heard during a full inspiration, but very distinctly after inspiration, or with expiration. It takes the place of the first sound at the spot where it is heard, but is followed by a distinct second sound. When the patient is breathing quickly, and the heart's action excited, it is best distinguished. It is not always of equal distinctness or of equal pitch, but it is not transitory, as it may be observed extending over a long space of time. The sounds of the heart are not influenced by it. They are heard with the usual clearness at the apex, immediately above the ensiform cartilage; at the third rib, or second intercostal space, on the right side: at midsternum; and even at the third costal cartilage and edge of sternum, on the left side. At this latter situation the second sound is distinctly heard; the occurrence of the first is more difficult to perceive and more doubtful."

In the course of this inquiry two points arise on which some light may be thrown by a comparison with phenomena in health. The blowing sound took the place of the first sound over the pulmonary artery. What are the natural sounds heard there, and how produced? Secondly, full inspiration prevented the sound from being heard. What is the effect of full inspiration on the sounds of the artery? In answer to these questions Dr. da Costa proceeds:

"For the purposes of an accurate standard in examinations of cardiac affections, I have studied and compared with each other, with care, the heart-sounds in the different portions of the organ, and also the effect of the respiration on them. I shall insert here as much as is relevant to the subject of the consideration of sounds in the pulmonary artery, merely adding that the usual clinical positions have been selected—the second intercostal space near the sternum, on the right side, for the aorta; on the left, for the pulmonary artery; whilst the sounds at the

apex have been studied a little above the left apex, and above the ensiform cartilage. Where the term apex merely is used, it is meant to apply to the apex beat on the left side of the heart, between the fifth and sixth ribs.

"It is seen, from an examination of twenty cases, that the first sound of the pulmonary artery is usually dull, of low pitch, and, in certain cases, more like a vibration than a sound, or so indistinct as hardly to be perceptible. Compared with the first sound of the aorta, it is equal in a certain number of instances (it was in six out of eighteen in the table); but when it differs (in eleven out of eighteen), it is noticed to be less distinct, less sharp, although it is in some persons a rather longer sound. Compared with the first sound at the apex of the heart, it lacks the weighty, prolonged, marked character of this sound. If the stethoscope be carried up from the apex of the heart to the second interspace, the difference is observed to be very marked; the change of sound occurs almost abruptly. It seemed to me, also, as if a decided change in pitch took place; but I do not wish to speak too positively on this point, as it is a very difficult matter to distinguish changes of pitch in sounds, both of which are dull, and one very indistinct. Compared with the first sound over the right ventricle (above the ensiform cartilage), the first pulmonic is of a much duller character, and decidedly less sharp, and not of as high a pitch. In this respect the difference—not merely in the above observations, but in others I have made—was noticed as much more decided than between it and the first sound over the left ventricle; the one on the right side being, although less strong and shorter, usually clearer, and of higher pitch even than this.

"These analyses, showing that the sounds of the heart, listened to in different positions, differ in character, are thus in favour of the view that the first sound of the heart, as heard over the arteries, is not merely a transmitted sound, but is one which—to a great extent, if not entirely—is generated by the coats of the vessels themselves during their diastole. They would further tend to show that the first sound of the heart, as heard over the apex on the right side, is not transmitted from one side to the other, but is formed by each ventricle separately.

"As regards the second sound in the pulmonary artery, as compared with the second aortic sound, it need only here be said that the latter is usually far sharper and more accentuated. Out of the eighteen cases compared, it was stronger in fifteen, equal in two, less marked only in one.

"A second point with reference to the blowing sound capable of being somewhat elucidated by a comparison with health was, why was it inaudible during a full inspiration, and so marked in expiration? The cases given, by exhibiting what effect a full inspiration has on the sounds of the heart in health, will explain it, especially if, in addition, it be borne in mind that the sound is sometimes of the same pitch as the inspiratory murmur, and that in expiration the heart's action is quickened. Ten young adults were selected, whose hearts, as far as the extent of percussion, dulness, rhythm, and impulse, proved, were perfectly normal.

"These observations, which have been repeated on other cases with

similar results, show, then, the effect of a full inspiration on the heart sounds to be an almost entire disappearance of the sounds heard at the interspace between the second and third ribs on the left side, and a very great, but not as marked as, diminution of the sound at the aortic cartilage. At the apex, the first sound lessens very much, and becomes in some persons almost imperceptible; the second also is modified, but proportionally less than the first. Over the ensiform cartilage, the sound lessens least. Indeed, the second sound, in four cases out of ten, was distinctly increased; and in two cases, in which the first sound became much more indistinct, the second was heard almost unchanged. These changes were all noticed in those who were capable of taking a very full inspiration and holding it for some time, and were constant on repeated examination. I believe, although the proximity of the valves to each other renders it impossible to speak conclusively, that this accentuated second sound, heard at the spot mentioned, is that of the pulmonary valves, and has become more marked, owing to the fact that during a full inspiration, the heart's action is slower, more laboured, and the circulation of the lung probably interfered with; thus the pulmonary artery would be distended, and the backward stroke of the column of blood against the semilunar valves be more forcible.

"The fact that the heart-sounds are in a full inspiration less distinctly heard at points at which they were previously well perceived, may be explained by the lung being carried in front of the heart, which it does more on the left than the right side, and, to some extent, by the relative displacement which occurs during the act of inspiration. The apex, especially, is displaced; it moves down in some persons, by several inches, towards the pit of the stomach, and becomes almost imperceptible at its previous point of impulse. This displacement seems to me to be brought about, not only by the depression of the diaphragm, but by the pressure of the lung on the left side of the heart, a fact of which I have convinced me by other observations."

ART. 45.—*On the treatment of Purpura Hæmorrhagica by Tincture of Larch Bark.* By Dr. S. L. HARDY, Physician to the Hospital for Diseases of Children, Dublin.

(*Dublin Hosp. Gaz.*, Jan. 15, 1859.)

Dr. Hardy tells us that he has been long in the habit of using larch bark as a styptic and carminative tonic. The tincture is of a dark carmine colour, with an agreeable "pinic" smell. In taste it partakes that of the oleo-resins. "It is," says Dr. Moore, "one of the most elegant forms at our disposal of prescribing a terebinthinate."

CASE 1.—A boy, æt. 16 years, who had frequently been a patient at the Hospital for the Diseases of Children, was presented for treatment on September 6th. His appearance was that of extreme debility; he could scarcely walk; his pulse was exceedingly feeble, and his countenance pale and dejected. He was ordered the *Liquor Pernitratæ Ferri*, with good nourishing diet. Having continued this treatment until the 15th, it was perceived that his symptoms were much aggravated; and there now existed a very extensive crop of spots of purpura over his entire body; his skin was harsh, dry, and

contracted; his spirits greatly depressed; and altogether his general appearance was most unpromising. There had not been hæmorrhage from the gums or by stool. Tincture of larch bark was now substituted for the iron, in fifteen-drop doses, to be taken every two hours.

The immediate improvement which resulted from this medicine was most remarkable; all traces of purpura disappeared, and his strength became so perfectly restored, that on the 23d instant he ceased to require further treatment.

CASE 2.—E. C—, a female child, æt. 7 years, residing in a small and badly-ventilated house, was brought to the Hospital for the Diseases of Children, on the 10th of August, having been a week ill. The account given by her mother was, that she had bleeding from the nose and gums, with discharges of blood from the bowels, and had lost all her strength. Her appearance agreed most truly with this statement. There was great debility, with a look of general distress in her countenance, sometimes more forcibly expressed by a knitting of the brows. Her body and extremities were covered with a very extensive and well-marked crop of purpura; the gums were spongy, and easily made to bleed; and her tongue was coated with a thick fur.

Tincture of larch bark was now commenced, in doses of ten drops, three times daily, after two days increased to fifteen drops, and given more frequently. On the 21st it is reported—"All bleeding has ceased; the tongue is cleaning; the bowels regular; and strength greatly improved."

From this date, under the same treatment, this child's recovery progressed rapidly and most favorably.

For the following cases, in further illustration of this subject, I am indebted to my colleague, Dr. Moore.

CASE 3.—July 1st, 1858.—John M—, æt. 2 years, was brought to the Institution for Diseases of Children. He looked a soft, flabby child, and has had a cough for the last six months. On examination, I found a thick crop of purpura studded over his chest, arms, and abdomen; he was languid, with total loss of appetite, and was greatly annoyed with a soft rattling cough. He never passed blood by his bowels. As the child lived in a comparatively unhealthy part of the city, I directed that he should be at once removed to the country; or, if that was not convenient, that he should be kept as much as possible in the park during the day. I prescribed tincture of larch bark, eight drops to be given three times a-day, in lemonade; which latter beverage, well sweetened, he was allowed *ad libitum* during the day and night.

July 3d.—The spots fading; cough still troublesome.

5th.—Purpura scarcely perceptible; the child to leave for the country. Larch bark discontinued.

I have seen this boy frequently since; he has had no return of the purpura, and the cough has entirely ceased. His general appearance is much improved.

CASE 4.—*Case of Bleeding from the Gums, and general Cachexia.*—Matilda M—, æt. 5 years, was brought to the Hospital for Diseases of Children, 15th September last. She looked anæmic and puny, and for the last few days free bleeding from her gums had occurred, two or three times, during the day and night. On examining the state of her mouth, I found the tongue "too red," and gums spongy. I prescribed ten drops of the tincture of larch bark, to be taken in cold lemonade four times during the day; the girl to have cold lemonade to drink freely; her food to consist of vegetables and fruit.

September 18th.—The bleeding has only occurred once during the last two days. Continue the larch bark, ten drops three times a day. Lemonade and vegetable diet, as before.

21st.—General improvement in the patient; state of the mouth and gums much healthier. There has been no return of the bleeding; however, I thought it advisable to continue the larch bark a little longer, and prescribed gutt. viij twice daily, in a mixture of chlorate of potash. This treatment sufficed to confirm the previous amendment.

I looked carefully for petechiæ in this case, but could not discover any.

ART. 46.—*Dynamoscropy*. By Dr. J. M. J. GASTON.

(*Amer. Jour. of Med. Sciences*, Oct., 1858.)

“The basis of this process, which is a discovery of M. Collongues, is as follows: When one places in his ear the finger of a sick or a well person, he hears immediately a certain buzzing, which varies in character according to the physical condition of the individual. With this noise is heard, at irregular intervals, a sort of crisping sound that the author calls crackling or shrivelling. These buzzings and cracklings are evidently produced by the person whose finger is inserted into your ear, for if, instead of the finger of a living being, you put that of a corpse or some inanimate body, you will hear nothing. Now, these sounds of a character constantly varying, it is the office of dynamoscropy to investigate, and though the science is of very recent origin, it may acquire in future very great importance. The author has already established that, in the normal state, the buzzing, &c., is slow, continuous, and equal, and that, on the contrary, it is, in the pathological state, rapid, strong, and unrestrained. When it is tremulous, unequal, now sharp, now deep, it indicates a seriously morbid state, and the case is still worse when the buzzing becomes intermittent. When the extremity of the finger, introduced into the ear, gives rise to no buzzing, it is an index of approaching death, except, of course, in cases of epilepsy, catalepsy, apoplexy, &c., when the noise ceases, to reappear with returning consciousness.

“M. Collongues, having remarked that the buzzings were more sensible when a solid body, such as wood or steel, was interposed between the finger and the auditory canal, conceived an instrument to which he gave the name of dynamoscope, and by aid of which his new mode of auscultation may be applied to all parts of the body. By this means he has established that the cracklings are heard only at the extremities of the fingers and toes, that the buzzings vary in different localities of the body, and that on the death of the subject, they cease in the extremities some time before disappearing from the belly and chest. It is twelve to fifteen hours after dissolution ere the noise is entirely lost; after that time, the dynamoscope, applied upon any part of the body, gives no more evidence of sound than if tested upon any other inert mass of inorganic matter.” Dr. Gaston’s object is to direct attention to this process, to show that the buzzing is to be referred not to muscular vibration, but to the passage of the blood through the capillaries.

(D) CONCERNING THE ALIMENTARY SYSTEM.

ART. 47.—*Ulceration and perforation of the Diaphragm in Peritonitis.*
By Dr. E. BONAMY.

(Archiv. Gén. de Méd., Nov., 1858.)

CASE.—A man, æt. 27, admitted into the Hôtel Dieu, at Nantes, with symptoms of idiopathic peritonitis. Nineteen days afterwards an abscess burst at the umbilicus; on the 22d day there was acute pain in the right side of the chest; on the day following there was frequent cough and purulent expectoration—the matter being more offensive than that which escaped from the umbilicus, and yet very like it in odour. In the inferior third of the right side of the thorax, behind, a profound and somewhat faint and amphoric souffle was heard, which seemed to proceed from the depths of the abdomen. From this time the patient got worse and worse, until the twenty-eighth day of his malady, when he sank, the only noteworthy circumstance being, that on the evening before his death each act of respiration was accompanied by a loud gurgling sound in the abdomen, and by the escape of air from the fistula at the umbilicus.

On examination after death there were false membranes and other signs of inflammation in several parts of the peritoneum. The right pleura communicated with the abdominal cavity by a large fistulous opening which compromised the diaphragm and a considerable portion of the liver corresponding to it. The right lung was pushed up as high as the seventh rib by a collection of purulent fluid in the pleura, and this fluid had overflowed into the air-passages through a broncho-pleural fistulous opening. And below the diaphragm was another large collection of matter in the space between the spleen and the grand cul-de-sac of the stomach, and the left extremity of the liver. This inferior collection of matter communicated with that in the pleura, through the opening in the diaphragm, and it had also another outlet through the fistula at the umbilicus, so that through these several channels air entering at the trachea could escape at the umbilicus.

ART. 48.—*On a new mode of treating severe Dyspepsia and Chronic Inflammation of the Stomach.* By ALEXANDER FLEMING, M.D.,
Senior Physician to the Queen's Hospital, Birmingham.

(Med. Times and Gaz., Jan., 1859.)

“In the medicinal treatment of affections of the stomach, I have long been convinced of the great importance of acting directly on the gastric mucous membrane. That, in fact, local treatment is here nearly as valuable as it is in affections of other mucous surfaces, as the eye, pharynx, vagina, and urethra. Hygienic rules and the management of the food are, for obvious reasons, very important in affections of the stomach, and will often cure mild cases without the help of medicine; but I am satisfied that, in the more severe and obstinate forms of chronic gastritis, the local medicinal treatment of the diseased mucous membrane has been unduly neglected—and that it contributes very powerfully to promote the cure.

“Of the several medicines which I have employed with a view to their

local action on the stomach, my experience gives the first place to nitrate of silver; and the observations I have now to make apply to this remedy. It is often given in pill. If this be made with bread-crumbs, the chloride of sodium in the bread converts the nitrate into the insoluble and comparatively inert chloride of silver. If made with gum or starch, the pill, on reaching the stomach, causes quickly the secretion of gastric juice, the chloride of sodium and muriatic acid of which again render the nitrate inert. It can have very little *local* action in the form of pill. I have for many years, therefore, given the crystallized nitrate dissolved in distilled water, in the proportion of from half a grain to four grains to the half-ounce. The dose is taken at bed-time on an empty stomach, and is repeated every night, every second, third, or fourth night, according to the severity of the disease. The stomach should be strictly empty—the patient recumbent—and he should be made to roll about immediately after taking the medicine. It is thus, before it suffers decomposition, brought into contact pretty freely with the mucous membrane, and gives, at the time and subsequently, evidence of its local action. In many cases, this mode of using the remedy suffices, in conjunction with other means, to effect a cure.

“But this method of exhibiting the medicine is not equal to the cure of some of the severer forms of dyspepsia and chronic gastritis; and in these I have, for the last four years, endeavoured to act more generally and efficiently on the mucous surface by injecting the solution into the stomach. I employ a strong brass syringe and flexible tube, one eighth of an inch in bore, the gastric end of which has a number of holes so directed that the fluid is thrown in a circular shower outwards and upwards on the walls of the stomach. The injection is made by dissolving from one to four grains of the nitrate in three ounces of distilled water. The operation is for the most part managed easily. Sometimes it causes nausea and retching—oftener not. It excites at first an enduring and grateful sense of coolness in the stomach, and subsequently there are felt pricking and sharp painful sensations, but of a different nature from the pains of the disease. Sometimes one injection is enough, but I have more frequently had to repeat it two, three, or more times.

“During the employment of the injections the patient takes three times a day and before food a little morphia, or chloric ether, or Indian hemp, in plain or cinnamon water. He is confined to small and frequent meals of milk, and as he gets better this is thickened with arrowroot or tapioca, and he is very gradually introduced to a nourishing and easily digestible diet. Counter-irritation to the epigastrium, nitrate of bismuth, oxide of silver, gentle tonics, &c., are employed when indicated.

“Of the thorough efficiency of this mode of acting on the mucous surface of the stomach, and of its power in promoting the cure, my experience, so far as it goes, is very decided. Although it is now four years since I first tried injection, I have not used it in more than ten cases. I have always, in the first instance, employed the simpler method already described, and resorted to injection only as a last resource; but its greater efficiency would, I feel certain, justify its employment in many of the less severe cases, and give more thorough and speedy cures. It is not my purpose, at present, to consider the intimate nature of the mode of cure, or the manner in which the nitrate of silver substitutes

healthy for diseased action in the inflamed gastric membrane. I must reserve that interesting question, and the detailed narrative of cases, for another opportunity."

ART. 49.—*Large flake of the epithelial coat of the Stomach vomited in a case of Scarlatina.* By Dr. BEALE, Physician to King's College Hospital, &c.

(*Dr. Beale's Archiv. of Med.*, No. III, 1858.)

The portion rejected was about the size of the palm of the hand. It evidently consisted of the epithelial coat of the stomach. The rugæ were very distinct in every part, and under the microscope only epithelial cells and mucus could be detected. There were many smaller shreds of the same nature rejected at the same time. At the post-mortem examination the walls of the stomach, at the cardiac extremity, especially in the upper part, were found to be very thin; and it was probably from this situation that the epithelium had been stripped off. The mucous membrane was much rejected, and covered with a very thin epithelial layer. Partial reparation of the lesion had therefore taken place, as would have been expected, since the patient lived three days after vomiting the mass above referred to. The epithelial layer, and a portion of the coats of the stomach, have been preserved.

CASE.—M. K—, æt. 24, admitted into King's College Hospital, August 20th, under Dr. Beale, who was attending for Dr. Todd. The patient had latterly been employed in the ward in which there were two cases of scarlatina, both slight ones. Previously to her present illness she had always enjoyed excellent health. On August 19th she felt feverish, and generally ill.

August 20th.—This morning she complained of headache and sore throat, and vomited occasionally. Her face, chest, and arms were completely covered with the rash of scarlatina. The tonsils, pharynx, and palate were of a deep red colour, and the tonsils much enlarged; skin hot and dry; appetite bad; bowels freely open; P. 108, R. 24.

21st.—Was very much purged during the night, and vomited several times; the vomiting being immediately excited by the beef tea. During the night she took ʒiij of brandy. This morning she complained of pain and tenderness over the epigastrium, the pain being increased by food. Her throat was more painful, and the left tonsil was ulcerated. The rash had spread over the whole body, and the conjunctivæ were inflamed; P. 120, feebler; R. 24; urine not albuminous.

Evening.—The vomiting and purging had continued during the day, and she had kept down neither food nor medicine; she was very restless, and a little delirious; pulse 122, feeble.

22d.—The bowels had not been opened since the administration of an opiate enema. She still vomited occasionally after taking her food, but retained the brandy. She slept pretty well, but seemed weaker this morning. The eruption continued out over the whole body, and the throat was more ulcerated: P. 134, R. 36.

23d.—She continued to vomit occasionally during yesterday afternoon, and in the night, but did not reject the iced brandy. In the night she slept badly, and was delirious. This morning, about 7 a.m., she vomited up, with some

bile, a piece of the epithelial coat of the stomach, about the size of the palm of the hand. Since this time the pain and tenderness of the stomach increased, and she vomited every thing she took. P. 134, R. 36.

Evening.—She retained the enemata, and had not vomited again. She continued to complain of pain in the epigastrium, where there was great tenderness on pressure. She was very restless, and wandered a good deal. P. 130.

24th.—She slept a little during the night, and retained the enemata. There was less pain and tenderness in the epigastrium. She complained much of thirst, and wandered a little. The eruption was fading from the face. Urine natural.

25th.—She was very delirious during the night, and got no sleep. There was less tenderness in the epigastrium, and she did not complain of pain after taking the brandy and water. At about 7 a.m., two and a half hours after having an enema, she had a very profuse evacuation, consisting partly of the altered enema and partly of very fetid feculent matter. Her pupils were much contracted, and she was very deaf. P. 108.

Evening.—She continued much in the same state all day, and retained the bread and milk and brandy. At night she was very delirious, and tried to get out of bed repeatedly. Pupils extremely contracted. P. 123, feeble.

26th.—She got no sleep during the night, and continued very delirious; towards morning she got much weaker; and when seen at 9 a.m. was in a state of low muttering delirium; picking at the bedclothes. P. 124, very feeble. She now had brandy given her every five minutes, but gradually sank, and died at 4 p.m.

Post-mortem, 24 hours after Death.—The lungs, liver, and kidneys were much congested; the other abdominal and thoracic organs appeared healthy, with the exception of the stomach. The mucous membrane of the stomach and duodenum were much congested, but there was no extravasation of blood in any part. The muscular coat, towards the pyloric extremity, was firmly contracted, and the mucous membrane thrown into rugæ; but the cardiac portion was relaxed, and the coats in this region seemed very thin; the mucous membrane was not thrown into rugæ.

ART. 50.—*Prolonged arrest of a foreign body in the Stomach.*

By Dr. ADLER.

(*Preuss. Ver. Zeitung*, No. 15, 1857; and *Archiv. Gén. de Méd.*, Feb., 1859.)

CASE.—A child, æt. 3, swallowed a small copper coin, and suffered for some days from vomitings and pain in the stomach. Three months later a morsel of food stuck in the throat, and death seemed about to happen, when, after sharp shakings and smackings on the back, the morsel of bread was ejected by vomiting, and along with it the coin, surrounded by a considerable quantity of grayish, viscid mucus. The coin in question had been in the stomach 102 days; it was blackish in colour, and the effigy upon it was almost effaced.

ART. 51.—*On the internal employment of Carbonic Acid in the treatment of Dyspepsia.* By Mr. G——.

(*Med. Times and Gazette*, Feb. 12, 1859.)

“I have been a sufferer,” says this gentleman, “for many years past

from chronic derangement of the stomach of an aggravated character, accompanied by a depression of the nervous system so severe as to make my life one of great wretchedness. To those who know from actual experience what these complaints really are, I need not describe my symptoms further. To those who know them not I will merely say—be thankful that you have escaped a fate so miserable.

“I have tried the regular doctors; I have tried Dr. Dickson, who has abused the regular doctors; I have tried the cold-water cure for six months; I have tried the grape cure at Meran in the Tyrol; I have been to Tunbridge Wells, Vichy, Carlsbad, Saratoga, and Kissengen. At each of these places I was under the care of a local physician of reputation. After all these things I was still a dyspeptic.

“I have been twice to Carlsbad, and I have drunk its waters, and used its baths long and perseveringly without any perceptible benefit. I have been three times to Kissengen. From my first and second visits I derived little, if any, benefit, owing, I believe, to having drunk an excess of the waters. Upon my third visit, in May, 1858, acting upon a suggestion of my own, I greatly reduced the quantity of water, and with advantage. The improvement in my health, it is true, was not very great, but it was an improvement, and that to me was encouraging. At the same time I felt persuaded that my perfect recovery would be a tedious one, extending perhaps over some years. I intended to have returned to Kissengen in the autumn, but private circumstances occurred which prevented me carrying out this plan. As a substitute for it I drank the Kissengen waters in England. I experienced some benefit from this course, but not so much as I had expected. I was somewhat disappointed at this result, and one day, while speculating as to its cause, I was induced to infer that the views which the faculty entertained touching the Vichy, the Carlsbad, the Kissengen, and their cognate waters, were altogether erroneous; that the efficacy of such waters depended neither upon alkalies nor metallic oxides, but upon carbonic acid; and that the right agent to employ would be pure water acidulated by means of carbonic acid gas.

“Shortly after this I commenced an experiment upon myself with water strongly impregnated with the above-mentioned gas, and within less than four weeks’ time my health was more improved than it was by my three visits to Kissengen. The improvement in my health is, indeed, now so decided that I confidently expect a few courses more of the carbonic acid-water will completely free me from my old complaints.

“The following directions embody the result of my present experience :

“The carbonic acid-water is to be taken before breakfast.

“Let the patient take a tumblerful of the water, and then walk briskly for half an hour. At the end of this time another tumblerful of the water is to be taken, and then another half-hour’s walk.

“This course is to be continued for a month.

“A discontinuance of the water for a month is then to take place.

“At the end of this time the water is to be begun again, and to be continued for a month, and then to be discontinued for a month, and so on until a cure is effected.”

ART. 52.—*On a new remedy for Dysentery.* By Mr. WM. KERR.

(Indian Annals of Med. Science, July, 1858.)

Mr. Kerr speaks highly of the efficacy of the following *olla podrida* in dysentery, but his statements are not very circumstantial or intelligible. He seems with little rhyme or reason to have added or taken away first one thing and then another. His formula is—

One of the Scrophulariæ .	. Digitalis purpurea.
Two of the Solanææ .	. { Datura stramonium.
	. { Solanum dulcamare.
Three of the Umbelliferæ .	. { Sium lineare.
	. { Cicutæ maculata.
	. { Angelica atropurpurea.
When necessary, one of the products of the Papaveracea .	. } Opium.

These ingredients are mixed in equal quantities, and the dose is three and a-half grains, half a dozen times a day, if necessary.

ART. 53.—*On the exhibition of Raw Meat in Diarrhœa.*

By M. TROUSSEAU.

(Jour. of Pract. Med. and Surg., Paris; and Glasgow Med. Journal, April, 1859.)

The meat best adapted to the purpose is the fillet of beef. Some patients, however, prefer the centre part of mutton chops. It should be cut fine, pounded in a mortar, and strained through a sieve or a cullender. The pulp thus separated from the cellular texture of the muscular substance is then gathered with a knife and rolled in salt, or powdered sugar, or mixed with currant-jam.

One of M. Trousseau's grand-children would take it only when mixed with racahout, a farinaceous compound of cocoa, ground-rice, and potato-flour, sweetened and flavoured with vanilla. M. Trousseau causes it sometimes to be rolled into small salted balls of the size of a hazel-nut, or in little oblong gobbets, which may be administered in soup to the number of thirty or forty, equivalent to four or five ounces of meat-pulp. In grown persons, and particularly with ladies, the physician will probably meet with a repugnance, which he must overcome by concealing the repugnant character of the medication. For this purpose some appearance of cooking may be imparted to the food, by exposing a thick slice of the meat for twenty minutes to the action of a brisk fire: its surface is thus roasted, the interior parts remaining raw, and being then treated as we have said. M. Trousseau has thus caused to be prepared by M. Mialhe (one of the principal apothecaries of Paris), meat-pulp, combined with confection of roses, destined for delicate stomachs, which is taken without disgust, and even with pleasure, under the agreeable denomination of Damascene preserve.

In children the dose of raw meat the first day should not exceed $2\frac{1}{2}$ dr. in four meals; it may be doubled on the second day, and on the third attain eight drachms, and so on, without any other additional food than

albuminous water. It is easy to measure with precision the quantity administered daily, by means of a small balance and the current coins, the weight of which is well known, the franc being equivalent to one drachm, and the five-franc piece to six drachms. The dose may be carried as far as ten or twelve ounces, and the children gradually recover their good looks, their plumpness and spirits; at the end of a month or six weeks, when diarrhœa has entirely ceased, the quantity of raw meat can be gradually decreased, and broth or underdone eggs can be substituted, so as to reduce the dose of meat to three or four ounces daily.

It is necessary to be aware that at first, when already the nature and abundance of the diarrhœa has undergone a favorable change, the motions are red and fetid. In one of the little Mulhouse patients we above referred to, this animal diet appeared to have occasioned the development of tape-worm, a parasite commonly met with in Abyssinia, where the natives feed on raw meat. But this kind of nutriment, not being so long persevered in, generally, as was the case in the instance of the little girl alluded to, this circumstance must be considered exceptional, and cannot counterbalance the decided benefits yielded by the Russian method of treatment, in cases of chronic disturbance of the bowels, and especially in the unconquerable diarrhœa which children are subject to in their second year.

ART. 54.—*On cord-like Consolidation of the Intestine.*

By Dr. BENNET DOWLER.

(Pamphlet.)

In the cord-like contraction of the large intestine, the sacculated or pouched cavities are obliterated so as to lose their natural configuration both within and without the tube. The longitudinal bands of the muscular coat or the length of the bowel appears somewhat shortened, although actual measurements are still wanting to decide this point positively. The contraction of the circular muscular fibres, by obliterating the convex salient bulgings, proper to the normal condition, would reduce the apparent length of the intestine, even though the longitudinal muscular bands (the *ligamenta coli*) might be in no degree shortened.

This remarkable lesion, though probably often secondary, like many others, appears to be similar neither to the transient spasmodic-contractions which probably occur in colic, nor to the permanent stricture, both of which are usually very restricted in extent, while this cordiform lesion includes, in many cases, the entire cæcum and colon.

It is possibly a primary lesion in some cases of yellow fever and in other diseases. In some persons not habitually costive, that condition precedes or accompanies the attack of yellow fever. From the induration, dryness, and rounded form of the fæces, and the firm contraction and exact adaptation of the intestine to these configurations, this lesion must occur anterior to the latter stage of the disease, as many post-mortem examinations indicate. In any case, it is a prominent organic change, and, consequently, implies a previous functional derangement of

a significant character, particularly in reference to the equilibrium of the circulation.

“This lesion is more common in yellow fever than in any other fever. It is also found in congestive, and even in typhoid. I have found it in bilious-remitting fever, also. It is probably, in a modified form, often the cause of obstinate constipation, colic, ileus, and may be superinduced by lead-poisoning. This contraction, it may reasonably be supposed, simulates scirrhus strictures, invaginations and fæcal and calculous concretions. In all of these conditions the diagnostic symptoms are, unfortunately, very obscure and equivocal, so far as the mere contraction of the intestinal calibre is concerned.”

CASE.—“Large man from Louisville: the cæcum, colon, and rectum contracted to the size of the thumb, scarcely pervious to the scissors, containing nothing but a handful of small, dry, inodorous, friable, black crumbs or scybalæ, from the size of a pea to a hazel-nut; the whole of the large intestine being firm, strong, elastic, thickened, blanched, bloodless, and scarcely moist within. The following facts are mentioned for comparison: mouth and gullet full of blood; stomach small, blanched, four ounces of black-vomit, without any bloody tinge; jejunum, blood and black vomit; the upper third of the ileum gray chylous paste, the residue black-vomit again.”

ART. 55.—*Relative value of different Anthelmintics in the treatment of Tenia.* By Dr. PEACOCK, Assistant-Physician to St. Thomas's Hospital.

(*Med. Times and Gaz.*, Nov. 6, 1858.)

The following is a brief summary of a series of cases in which different anthelmintics had been employed against tapeworm. The patients were all treated by Dr. Peacock, in the out-patients' department at St. Thomas's Hospital, and we are indebted to him for access to the detailed notes upon which the statements are founded:

As a general result of his experience both in public and private, Dr. Peacock states that he gives preference to the oil of male fern before all other remedies, and that he holds the kousso in very light estimation indeed. It appears that of the hospital cases respecting which notes have been preserved, the fern oil was given in thirty-five. Of these, in sixteen no other remedy had been previously tried, and in this group the result was always satisfactory, the animal being expelled in a dead or dying state. In seven cases the oil was given after the partially successful use of kousso, and in all these more of the worm was brought away. In three, after partial success by pomegranate bark, the oil brought away other portions of the parasite, and in one a like result was obtained after the use of the turpentine draught. In six cases in which the oil was used, either the result was not satisfactory, or the patient did not attend again. The dose of the oil given was from half a drachm to a drachm and a half to children, and from a drachm to three drachms to adults.*

* We are informed that great care is necessary on the part of the dispenser, in order to avoid disappointment in the use of the oil of fern. Its ethereal solution, which is by

The cases in which the kameela was given are seven. In five of these no other remedy has been previously tried, and in all these portions of worm (generally quite alive) were expelled. In one the expulsion of worm was caused after koussou had been tried without effect, and in the fifth, which was under similar circumstances, a like negative result followed its use also. In two cases after the successful employment of the kameela, the oil of fern was employed without procuring the expulsion of any more of the worm. The dose of kameela prescribed was from half a drachm to a drachm for children, and from one to three drachms to adults.

It would from the above facts appear that kameela is more efficient than koussou, but that it must rank as a vermifuge rather than a true vermicide. After the fern oil the animal is usually voided dead. An important statement with regard to the comparative value of kameela, is made by Mr. Henry Callaway, formerly of Finsbury Circus, but now a medical missionary amongst the Zulus. The kameela is the native remedy among the aborigines; but, in a letter to the 'Pharmaceutical Journal,' Mr. Callaway states, that from experience they have learned already to put much more confidence in "the white man's dose." The latter consisted of turpentine and castor oil, the time-honoured remedy among ourselves. We are not able from Dr. Peacock's cases, to institute any comparison between turpentine and fern oil, and can only state that we believe he is supported by several other hospital physicians who have given much attention to this matter, in maintaining that the latter ought to stand *facile princeps* among our anthelmintic drugs.

As regards the economics of the question, which are important in hospital and union practice, it will, of course, be easily granted that all things considered the most efficient remedy will probably in the end prove the cheapest. A dose of castor oil and turpentine, undoubtedly, costs far less than any of the others. Next to it comes the koussou, which has as rapidly fallen in price as it has in general estimation. The kameela is, as yet, rather expensive, though not nearly so much so as the fern oil. A full dose of the last costs eightpence, of the kameela about fourpence, of the koussou threepence, and of the turpentine and castor oil not more than three-halfpence.

Kuchenmeister, in his 'Manual on Parasites' (Sydenham Society's edition), writes of the oil of turpentine as follows: "As has already been remarked, the touchstone of a remedy for tapeworm is not whether it expels *bothriocephalus latus* or *tænia solium*, but whether it is also capable of effecting this with *t. medio-canellata*. That oil of turpentine is efficacious in the latter case I can prove at any time; for the finest specimen of *tænia med.* that I ever saw was expelled by it. In general also it acts pretty rapidly. Lastly, it has also the advantage that it expels the worm entire." Of the koussou he writes, "For my part I have always been more or less unlucky with this remedy. . . . I have generally seen the worm expelled in innumerable fragments. . . . I have never found the head. In one case I detected fragments in the evacuations

far its best preparation, on standing deposits its resinous principle. A prolonged shaking is necessary to secure readmixture. Unless the dispenser pay more than usual attention to this matter, the patient is very likely to get a dose which is but little more than ether.

for three months." Professor Martius of Erlangen, who also has used koussou largely, never saw the head brought away. Of the male fern, Kuchenmeister states: "This remedy, which will always maintain its renown against the *bothriocephali*, appears hardly to maintain its reputation with regard to *tænia*." The kameela he had not tried.

Of the desirability of having the intestinal canal as empty as may be before giving anthelmintics, most practitioners are aware. To administer them fasting in the morning is usually thought sufficient, but in cases where difficulty has been encountered in destroying the animal it may be well, as an introductory measure, to give a sharp purgative.

ART. 56.—*Recovery after repeated operations of Paracentesis for Ascites.*
By Dr. BANKS, King's Professor of Physic, and Physician to the Whitworth Hospital.

(*Dublin Hospital Gazette*, Feb. 1, and March 1, 1859.)

The two following cases are remarkable on account of their unusual termination. These stand, indeed, almost alone in this respect.

CASE I.—A woman, æt. 32, was admitted into the Whitworth Hospital in the month of March, 1857; she had been, on former occasions, a patient in the hospital, but the ailments under which she laboured were unimportant, and bore no relation to the disease which forms the subject of the present notice. Her health was generally good; she was fifteen years married, and had borne six children; and, being extremely poor, she had suffered many privations. Her habits had always been strictly temperate. On admission, she stated that for many weeks she had laboured under the illness which had finally constrained her to seek hospital relief. The earliest deviation from her usual health was a pain in the right side, and about the same time she perceived an alteration in the colour of her skin; and she, moreover, observed that she was gradually losing flesh and strength. The quantity of urine passed was notably diminished; and slowly, but steadily and uninterruptedly, the abdomen increased in size, until at length it became so large as seriously to incommode her from its great bulk; and from the difficulty of breathing, caused by an extraordinary exertion, she found it impossible any longer to pursue her occupations, which were usually of a most arduous nature. The face was thin, as were also the extremities; the conjunctiva and the skin generally presented an icteroid hue. The abdomen was as large as that of a woman who had completed the full period of utero-gestation, and its surface was traversed by numerous veins of considerable magnitude. Fluctuation was distinctly perceptible, and the result of a minute examination of the abdomen left no doubt of the nature of the disease; the conclusion arrived at being, that the dropsy was non-encysted, and that the hydrophic effusion depended on hepatic disease. In coming to this opinion, we were aided by the exclusion of the many other causes which are potent in the production of serous effusions into the peritoneal cavity. The history of the case precluded the idea of the dropsy originating in inflammatory action of the peritoneum. We failed to obtain any information which could lead to the supposition of the effusion having taken place subsequently to the sudden impression of cold, or to the suppression of a flux to which the system had become habituated. She had not recently been the subject of fever of any

kind, nor had she suffered from any form of hæmorrhage. The urine was, in quantity, far below the standard of health, rather high-coloured, and deposited, on cooling, a copious sediment of the urates and purpurates. No trace of albumen could be detected, but the nitric-acid test afforded evidence of the presence of a minute but appreciable quantity of bile. The heart's action was perfectly healthy; not the slightest ground for suspecting any lesion existed, and the only circumstance connected with this organ which calls for remark, was the fact of its impulse being felt about an inch and a half above its usual site—the abdominal effusion having encroached upon the thoracic cavity, and consequently pushed the heart upwards, the altered position, as I have frequently remarked, having no influence on the due performance of its functions. The lower extremities and the face were free from the slightest trace of anasarca. This alone was sufficient to exclude cardiac or renal disease, and even obliteration of the vena cava could not, for obvious reasons, be entertained for a moment when the etiology of the disease engaged our attention.

Even without closely interrogating the other organs, upon lesions of which serous effusion of the abdomen might depend, the practical physician could have little hesitation in arriving at the opinion that the dropsy was of the obstructive form, and that the organ at fault was the liver. The presence, not alone of bile in the urine, and the semi-jaundiced hue of the skin, but, in addition, the fact of the urine containing purpurates, added confirmation to this view of the case. The lesion of one or other of the organs connected with the portal system—and, with extreme probability, the liver—may be predicated whenever the renal secretion *constantly* exhibits pink or flesh-coloured deposits.

The dropsical distension was so great that the operation of paracentesis could not be postponed much longer, and it was accordingly performed on the 14th of March, 1857. The instrument selected was extremely small, the same size I am in the habit of using in performing the operation of thoracentesis. Twenty-six quarts of fluid, of a pale straw-colour, flowed away through the canula, and, on its removal, fluid continued to trickle through the minute orifice for the following twenty-four hours. After the evacuation of the peritoneal effusion, an examination was instituted to determine the state of the abdominal organs—an investigation which could not be satisfactorily performed in the distended state of the abdomen, prior to the abstraction of the fluid. The liver appeared to occupy the limits which are assigned to it in health; it was not smaller than natural—a state which, it must be confessed, was anticipated. There was no evidence of enlargement of the spleen. Almost all the symptoms which caused so much distress previous to the operation were removed or ameliorated. It was noticed particularly that the functions of the kidneys, which for some days had been nearly in abeyance, was now nearly restored to its pristine activity, and the urine approached in its characters more closely to that of health. The patient's condition became one of comparative comfort, and in a few days she requested to be discharged, from her anxiety to be again actively employed in the endeavour to support her family.

On the 15th of April she was again received into hospital, in nearly the same state as before; the operation of tapping was again resorted to, and precisely the same quantity of fluid, viz., twenty-six quarts, was drawn off. Again the operation was followed by marked relief to the symptoms which so severely harassed her.

The moment her strength was sufficiently restored, she left the hospital,

and, as before, returned to hard work, and to poor and insufficient nourishment. In the following month (May) the same scene was enacted; and, strange to say, the identical same quantity (twenty-six quarts) was the produce of the operation. In the next month the dropsical distension again demanded that the operation should be repeated, and twenty-seven quarts of serum, in all respects resembling that observed on the former occasions, were removed. The fluid again accumulated, and, in July, twenty-eight quarts were removed by tapping. In August the quantity was considerably greater than it had been on any of the previous occasions, amounting now to thirty-six quarts. It may here be noticed, that, up to the sixth operation, the peritoneal effusion was marked by a certain regularity, as to the rate of its accumulation, and as to the amount removed in the reiterated operations. In September, the periodicity hitherto observed was broken, the symptoms urgently requiring that relief be afforded twice in the course of the month; and on the first occasion twenty-six quarts, and on the second fifteen quarts, flowed away. She did not now rally so rapidly as before. Her strength was at a very low ebb; she exhibited a more marked amount of attenuation; and her restoration to her ordinary state, after the withdrawal of the fluid, seemed to be mainly retarded by the inability to retain food, nearly everything taken into the stomach being rejected by vomiting. The abdomen slowly increased in size; the umbilicus became prominent, and its integuments gradually thinned. In October it was found expedient to operate, and twenty-six quarts of fluid were drawn off. In November the operation was twice performed, and fifteen quarts on the first occasion, and sixteen on the second, came away. The umbilicus had been for some time gradually increasing in size, and now a protrusion of considerable magnitude existed, which increased on coughing or rising in bed. In the early part of December she was tapped, and twenty quarts drawn off, and in three days she left the hospital. The urine, at this period, was scanty, and presented the characters already alluded to—viz., it deposited abundance of the lithates and purpurates, and contained bile. She did not return to hospital as soon as she had been in the habit of doing, and, on applying for admission, she informs us that a little crust or scab, which had formed on the most prominent part of the umbilical protrusion, had given way, and twelve quarts of fluid came off. From this date (the 30th of December) until the 8th of January, 1858, there was a continual flow of colourless serum from the opening, which had been becoming less for the last three or four days, and on the 9th had become closed. The protrusion had almost disappeared, but a small abscess formed, which was opened, and exit given to about a tablespoonful of purulent matter. A second abscess formed, and now the umbilicus was surrounded by an erysipelatous redness, which extended downwards as far as the pubes. The abdomen during this time was growing larger, but it was remarked that the process of exudation was more slow, the quantity of effused fluid being much less than it had been formerly in the same space of time. The erysipelas gradually faded away, but it was observed that its presence did not excite any marked constitutional disturbance. From this time the operations were performed at shorter intervals than formerly, the abdomen never being permitted to attain any great size. This measure was adopted in some degree at her own request, from the experience of the great relief afforded her on each occasion. One exception there was, arising from her remaining out of hospital longer than usual.

The accumulation evidently, of late, had been much slower than before, and her health, with some interruptions, had improved. On the 14th of June, 1858, she was tapped for the last time—the twentieth. Since that

period there has been no dropsical effusion; and now, at the expiration of seven months, she is perfectly well; indeed, to use her own words, she "never was in better health."

The abdomen is large, but the most careful examination fails to discover any fluid in the peritoneal sac; and the size of the abdomen may fairly be attributed to the enormous distension of the walls repeatedly experienced during the long period intervening between the commencement and termination of the disease. The catamenia, which had been absent since the birth of her last child, two years since, and during the whole course of her illness, have returned.

The *exact* amount of fluid in this case cannot easily be calculated; but estimating what was removed by the operations of tapping, what followed the rupture of the umbilical protrusion, and what oozed away after the operations, I think we may conclude that more than 400 quarts escaped from the peritoneal sac. The fluid was a pale straw-colour, often containing a little bile; and the specific gravity was 1012.

CASE II.—A young woman, æt. 24, was received into the Whitworth Hospital on the 1st of February, 1858, labouring under general dropsy. A considerable amount of fluid existed in the cavity of the peritoneum, and the lower extremities were large and œdematous. The urine was pale, neither above nor below the ordinary standard in quantity, and contained a notable amount of albumen. She stated she had not been long ill, and she had not recently laboured under scarlatina or any other febrile disease. Her habits of life had been irregular, and she had been frequently exposed to cold. There could be no doubt of the nature of the disease, inasmuch as the absence of every sign and symptom indicative of hepatic or cardiac disease, and the presence of albumen in the urine, and the fact of its low density (1012), pointed to the kidneys as the organs to which the dropsy was to be attributed. The amount of peritoneal effusion was unusually large, considering that there was sufficient evidence of its being merely a part of general dropsy, and not traceable to obstructive disease of the liver. The young woman was weak, and presented the anæmic appearance so characteristic of renal disease. Soon after admission, the feeling of distension and the difficulty of breathing became so distressing, that it was determined to remove the fluid from the abdomen by tapping. The operation was followed by a marked alleviation of the most urgent symptoms, but it soon became apparent that the fluid was again accumulating in the peritoneal cavity. From the first operation to the date of her dismissal from the hospital, a period of seven months elapsed, during which she was tapped thirteen times—the average quantity of fluid removed at each operation being twelve quarts. The interval elapsing between each operation varied from nine to fourteen days. In addition to the drawing off the fluid from the abdomen, at intervals, iodide of potassium in small doses was administered; and this treatment was steadily pursued, with much advantage, for a long time. Ferruginous preparations were also given, and gradually the case assumed a more promising aspect than could have been expected, remembering the state of things which existed at the period of her admission into hospital.

It is unnecessary to enter into minute details of the progress of the case during the months she remained under observation; it will be sufficient for my present purpose to notice the fact, that at length the dropsical symptoms totally disappeared, the urine returned to its normal state, and the young woman left the hospital apparently restored to health. I have no knowledge as to whether she has remained so since or not, but she has been seen by one of our students, who reports that she was looking well; and I am persuaded

that, in the event of the disease having returned, she would have again sought admission into hospital.

ART. 57.—*Cases of acute yellow Atrophy of the Liver with Icterus.* By (1) Professor BAMBERGER, of Würzburg, and (2) Dr. MUHLIG, of Constantinople.

(1, *Gaz. Hébd.*, July 6, 1858; and 2, *Gaz. Méd. d'Orient*, March, 1858.)

Both these patients had been debilitated by previous disease, and acted upon by strong emotion, and no special circumstances can be pointed out as leading to the disease. The cause of the destruction of the cells Professor Bamberger considers to be acute parenchymatous inflammation.

1. *Professor Bamberger's Case.*

M. T—, æt. 28, had recovered some weeks ago from a pelvic abscess which had discharged through the vagina. She lived in trouble, and seemed to have been subjected to strong emotions on the first days of her new malady. On the 3d of October, 1856, she became icteric; she complained of cephalalgia, tinnitus aurium, prostration, and pain in the extremities. On the 6th the liver extended beyond the ribs, and its border could be easily felt on palpation. On the 9th it could not be reached except by thrusting the fingers deeply into the hypochondrium; also percussion proved a diminution of the volume of the liver. On the 15th evening delirium set in, then agitation, and on the 16th, the day on which the patient entered the hospital, she was in a comatose state. Lying on her back, she seemed to be unconscious of what was passing around her, and only answered in monosyllables, if questioned; sensibility was, however, retained; convulsive attacks from time to time; intense icterus of the skin; pulse 68, very small; temperature in the axilla, 35.5° R. Right hypochondrium not painful on pressure; percussion dull only on the external side of the hepatic region to the extent of seven centim., more internally somewhat tympanitic; to the left of the median line all dullness disappeared; the stools only little coloured; urine voided involuntarily, and of icteric tint. (Ether; wine; injections of vinegar; sinapisms; cold affusions.) A little improved on the 17th. On the 18th icterus more intense; depression more profound; pulse 70; temperature 36.2° . (Calomel; affusions.) Much agitation during the night. On the 19th grayish, involuntary stools; nearly absolute coma; hepatic dullness less extended than previously; pulse 84; temperature 36° ; hands cold, cyanotic; the patient has, nevertheless, some appetite. On the 20th the stools were quite colourless; agitation alternated with prostration. On the 21st nothing could awake the patient; the skin, if raised into a fold, would resume its place only slowly; a diphtheritic ulcer had formed on the interior side of the lower lip; sanguinolent sputa; pulse 120. Died the same evening.

Autopsy.—Icteric coloration of all the organs, with exception of the brain, which contained only little blood, but presented no other anomaly. The left heart and the vena cava contained liquid blood mixed with some soft clots. Underneath the endocardium of the left ventricle a great number of ecchymoses were observed; the spleen was voluminous; liver atrophied; the diminution of volume affected, particularly its left lobe, and the thickness of the organ, which, at a point of the right lobe, did not exceed eight centimetres. Its tissue was remarkably flaccid and pliable; its colour orange-yellow, mixed

with red in the left lobe. The lobules were nearly effaced, except toward the posterior border of the right lobe, which contained also more blood than the rest of the organ. The branches of the portal vein were empty, otherwise healthy; the hepatic ducts collapsed, and for the most part empty. The gall-bladder contained only half a teaspoonful of thick bile of greenish-gray colour; hepatic artery permeable. The microscopic examination showed that the lobules of the liver were preserved nearly everywhere. The cells, which occupied their centre, were much coloured by bile, while those at the periphery were, for the greater part, destroyed, and replaced by fatty and pigmentary granules. The contents of the large intestines were quite colourless, except in the region of the ileo-cæcal valve, where they were slightly coloured by bile. The chemical analysis of the different liquids and solids of the economy, by Professor Scherer, was made particularly to search for leucin and tyrosin. It was found that—1. The urine contained leucin, and biliary pigment; neither albumen, nor tyrosin, nor tauro- and glycocholic acid. 2. The blood: leucin more abundant in the venous blood than in that of the left heart; no tyrosin nor hypoxanthin. 3. The bile: taurocholic and glycocholic acid; neither tyrosin nor leucin. 4. The spleen: hypoxanthin and leucin, little tyrosin, traces of oxalate of lime. 5. The liver: much leucin, and tyrosin, and hypoxanthin.

2. *Dr. Mühlig's Case.*

An English woman, æt. 21, had suffered much of late from misery and grief. She had arrived at the last month of her first pregnancy, when her conjunctiva became icteric, and her temper more irritable than usual. She was delivered without accident, and had no bad symptoms for three days; but in the night of the fourth she was seized with violent agitation and delirium. A physician was called in, and prescribed an emetic; afterwards the patient was put under mesmeric influence. Dr. Mühlig saw her on the fourth day. He found her lying on her back, the eyelids closed, the pupil dilated and immoveable; the skin and conjunctiva coloured light yellow; expression and physiognomy apathetic; persistent trismus; from time to time hiccoughs, and slight flexing movements of the forearm. The patient was in profound coma; no sensibility betrayed on pressure in the hypochondriac regions. Involuntary evacuations; retention of urine; temperature of skin not sensibly augmented. Percussion gave a tympanitic sound in the epigastric, and an obtuse tympanitic in the hepatic region, over the inferior ribs, to an extent of about two fingers square; higher up dulness to the extent of about four centimetres. Purgative injections and sinapisms. Died the following night.

Autopsy.—Absence of cadaveric rigidity. Heart normal, containing some liquid blood and very friable clots. The liver was reduced to nearly two thirds of its normal volume, and was concealed in the depth of the right hypochondrium; its peritoneal coat was wrinkled; its left lobe represented merely a small appendix to the right lobe. The diminution of volume was remarkable, particularly in the thickness of the organ, which seemed flattened, with the borders flabby and hanging down. Its tissue was flaccid and without resistance, and presented a uniform light yellow colour, except in some points where it was purplish-red, and gave out neither blood nor bile. Gall-bladder distended by a grayish, flaky liquid; biliary ducts contracted but permeable; spleen enlarged and softened; stomach and large intestines contained grayish mucus, mixed with a blackish liquid resembling coffee-grounds, but there were no hæmorrhagic erosions present. On microscopic examination, the hepatic cells were completely destroyed; only a molecular, yellowish, uniform detritus, and some oil-globules were visible.

ART. 58.—*On the diagnosis of Tumours and Enlargement of the Spleen.*
By Dr. HARE, Assistant-Physician to University College Hospital.

(*British Med. Journ.*, Nov. 29, 1858.)

After some observations on the difficulties attendant on the diagnosis of abdominal tumours and intumescences generally, and after having pointed out briefly on what these difficulties depended, and how some of them, at least, might be overcome, the author proceeds to define the exact scope of the paper, and to point out how light is thrown on the subject by a knowledge of the etiology, pathology, morbid anatomy, &c., as well as of the physical signs of the diseases of the organs in question.

After very shortly alluding to—1, the anatomical peculiarities of the spleen, and its somatic relations to other organs under their ordinary conditions, the author proceeds to consider, 2, the physical signs it produces when in its normal state; 3, the different diseases of the organ which give rise to its enlargement; 4, the varieties in appearance, size, shape, and consistence which it may offer; 5, the physical signs presented by the organ when diseased; 6, the various abdominal tumours with which an enlarged spleen is likely to be confounded, and the physical signs and other conditions which aid in the diagnosis; 7, how far the precise kind of disease can be determined in any case of splenic enlargement.

In this abstract, we can but refer to a few of the points insisted upon by the author. He says that in a normal condition, the spleen never descends below the ribs, but that sometimes, in very thin subjects with lax abdominal walls, its lower edge may be detected by tucking, as it were, the fingers under the costal cartilages; the limits of the dulness on percussion to which it gives rise are defined, and the circumstances connected with other organs, &c., sometimes interfering with the ready determination of its physical signs. Amongst the diseased conditions increasing the volume of the organ, reference is made to congestion from various causes: inflammation; fibrinous deposits; hypertrophies, both of the red parenchyma and of the Malpighian bodies, the lardaceous spleen often associated with a similar condition of the liver, &c.; tubercles; cancer; cysts of different kinds, &c. It is not very uncommon to find the spleen weighing from two to three, up to twelve or eighteen pounds, while instances are on record of its weighing forty and forty-five pounds. The different modes of physical exploration are described, the chief reliance being placed, as in the case of almost all abdominal tumours, on palpation and percussion. The signs produced by the organ in its various degrees of enlargement are alluded to, much value, in a diagnostic point of view, being attached to the detection of the thin anterior edge of the organ, and its notched condition; to the absence of intestines in front of the organ; to the superficial character of the tumour; to its smooth, somewhat convex, outer surface; to a certain degree of mobility of the mass; to the "splenic murmur," when this is present, though it is frequently absent. The splenic dulness on percussion is often very great, but sometimes, when the organ is thin, though enlarged, the resonance of the intestines underneath may be

transmitted through the tumour, and a certain amount of resonance over the edges of the mass is by no means uncommon. The detrusion of other organs by the enlarged spleen, and the physical signs thus caused are likewise referred to.

The author then speaks of the increase of the difficulty of diagnosis produced by the simultaneous enlargement of other viscera, particularly the liver and the left kidney; often, too, it is a question whether a given tumour be splenic or be referable to one of the organs just named; and certain conditions (thickening, &c.) of the stomach, tumours of the omentum, and even of the ovary, &c., have been mistaken for enlarged spleens. The kidney tumours have a less sharp edge than splenic ones, and usually have intestine, or, if very large, at least the colon, in front. Dr. Hare once met with a murmur in a renal tumour, but only once; it was synchronous with the systole of the heart, or nearly so, and it was in the tumour, not transmitted from the aorta: on the other hand, a murmur in a splenic tumour is not very uncommon. The condition of the urine also frequently assists in determining the character of a renal, in contradistinction to a splenic tumour. With regard to mobility as a diagnostic sign between these organs when enlarged, the author gives it as his opinion, that though splenic tumours are usually more moveable than renal ones, tumours of the latter kind are often more mobile than is usually supposed, and more so than they are usually stated to be in books. Being able to trace a transverse line of dulness from the left to the right hypochondrium, and the detection of the notch between its two lobes, tend to point out the liver as the seat of enlargement, either (as the case may be) coincidently with, or independently of, the existence of splenic tumour.

The connexion between certain general states of the system, or special symptoms (such as typhus fever, ague, leucocythæmia, &c.) is next discussed, and it is shown, that by a consideration of the physical signs, together with the general symptoms, &c., a perfectly correct diagnosis might often be arrived at, not only as regards the spleen being the organ in question affected, but also as to the exact character of the disease itself.

(E) CONCERNING THE GENITO-URINARY SYSTEM.

ART. 59.—*On the value of Albuminuria as a symptom of Kidney Disease.*
By Dr. PARKES, Professor of Clinical Medicine, in University College, London.

(*Medical Times and Gazette*, Jan. 1, 1859.)

Valuable tables on the presence or absence of albumen (derived from the kidneys) in the urine in various diseases, have been given by Martin-Solon, Finger, Haywood, Johnson, and others. In order to test the accuracy of the usual statements, and especially to draw a stronger distinction between temporary and permanent albuminuria, Dr. Parkes has lately analysed those cases of his adult patients in University

College Hospital in which the urine was examined carefully and daily for a sufficient length of time to enable it to be said with perfect accuracy, that albumen was or was not present during the whole course of the disease, or at any time of it. Cases of cystitis and vaginitis were not included. By the term "temporary albuminuria" he implies a case in which, after lasting for some days or even weeks, the albumen entirely disappeared for some time before the patient left hospital; and by permanent albuminuria, a case in which the albumen did not disappear during the time the patient was under observation; this time being generally very long and always many days. University College Hospital is a general hospital, into which cases of smallpox and scarlatina are seldom admitted. He excludes from consideration all cases of cholera and of pregnancy.

TABLE I.

---	Total number of cases.	Urine not albuminous at any time.	Urine temporarily albuminous.	Urine permanently albuminous.
Men	170	124	21	25
Women	133	103	16	14
Reduced to a per-centage, this table is as follows :				
In every 100 men admitted into the medical ward of a general hos- pital in London		72·94	12·35	14·71
In every 100 women admitted into a general hospital in London . .		77·44	12·03	10·53

The proportion of cases of temporary albuminuria is therefore almost the same in the two sexes; *i.e.*, the causes producing the albumen are independent of sex. The proportion of cases of permanent albuminuria is greater by rather more than 4 per cent. in males, on account of the greater prevalence of organic kidney disease in male hospital patients in London.

(a) *Temporary albuminuria.*

The causes of the temporary albuminuria were—

TABLE II.

Disease.	Men.			Women.		
	Total number of cases.	Temporary albuminuria.	No albuminuria.	Total number of cases.	Temporary albuminuria.	No albuminuria.
Paraplegia (spinal)	2	1	1
Hemiplegia (cerebral hæm.) . .	3	1	2	1	1	...
Phthisis (chronic)	17	1	16	11	...	11
Pleurisy (acute)	11	1	10	6	...	6
Bronchitis (acute) lungs otherwise healthy	3	1	2	3	...	3
Bronchitis (acute in emphysematous lungs)	6	1	5	3	1	2
Bronchitis (tuberculous lungs)	1	1	...
Pneumonia (acute lobar)	7	3	4	3	3	...
Heart disease, hypertrophy, dilatation, and valvular affections .	6	1	5	11	1	10
Acute morbus Brightii	3	3
Acute rheumatism	8	2	6	11	2	9
Subacute rheumatism	8	1	7
Purpura hæm.	1	1	...
Typhoid fever	10	2	8	9	3	6
Typhus	2	1	1
Variola	2	1	1	3	1	2
Scarlatina	1	1	...	1	1	...
Erysipelas (leg)	1	1
Total	88	21	67	66	16	50
Per-centage	23·86	76·14	...	24·24	75·76

The per-centage of this table (which includes all the diseases any case of which had the urine temporarily albuminous) shows, as in Table 1, that the effect of sex is not obvious in the production of this symptom.

The quantity of albumen was large in the pneumonic and acute renal cases; was in some quantity in the typhoid, variolous and scarlatinal cases; was very small in most of the rest. Blood on microscopic examination was observed only in three cases, in which the amount of albumen was small; but it may have been present in some others.

(b) *Permanent albuminuria.*

The causes of the albuminuria were—

TABLE III.

Disease.	Men. Albuminuria.	Women. Albuminuria.	Remarks.
Morbus Brightii (all forms)	16	9	
Encephaloid disease of kidney	1	...	
Cystic disease of kidney . .	1	...	
Leucocythemia with presumed lardaceous kidney disease	...	1	
Chronic phthisis and kidney disease on post-mortem examination	1	...	
Pleurisy with probable kidney disease	1	...	Casts and kidney-structures in sediment of urine.
Heart disease (hypertrophy) and valve affection . . .	2	2	Kidneys healthy in 3 cases on post-mortem examina- tion.
Hemiplegia from cerebral softening	1	Blood-corpuscles.
Pancreatic disease causing icterus	1	...	Amount of albumen very small.
Purpura hæmorrhage . . .	1	...	Case not fatal; nothing de- finite known about kidneys.
Typhoid fever	1	1	Blood-casts and establish- ment of decided morbus Brightii consecutive to the fever.
	25	14	

Therefore, in 32 out of 39 cases of permanent albuminuria, disease of the kidneys was either proved, or was rendered highly probable, by the presence of other symptoms. In three other cases kidney disease was not disproved; but as nothing was decidedly known, he is justified in excluding them from further consideration.

Permanent albuminuria indicated, then, kidney disease in 32 out of 36 cases; and if the heart diseases be excluded, it may be said that it indicated kidney disease invariably. Of late years the discovery of albuminuria in so many diseases has caused some scepticism as to the value of this symptom, but it would seem that the distinction of temporary and permanent albuminuria has not been drawn with sufficient care, and that if it be permanent, the significance of albuminuria is as great as it was supposed to be by the older writers on morbus Brightii.

A distinction between the temporary and permanent albuminuria was often probable even at the first examination, from the usually much smaller amount of albumen in the former class of cases, with the exception, however, of the pneumonic, specific febrile, and acute renal cases, in which the quantity was often large.

ART. 60.—*The identity of Scarlatinal Dropsy and acute Morbus Brightii.*
By Dr. BASHAM, Physician to the Westminster Hospital.

(*Lancet*, Jan. 1, 1859.)

The following case illustrates two important facts in the pathology of the kidneys. First, the relation which scarlatinal dropsy bears to acute morbus Brightii; and secondly, the character of the structural changes which at first impede, and ultimately destroy, the function of the kidney.

It was at one time doubted whether the renal disorder after scarlet fever had anything in common with those forms of disease described by the late lamented Dr. Bright, which the concurrent opinion of European pathologists now designates as morbus Brightii. It scarcely required the genius of a Rokitansky to identify them; yet that great pathologist has not hesitated to recognise in scarlet fever one of the most energetic causes of the acute form of Bright's disease. The symptoms during life are the same, and a microscopic examination of the renal structures establishes the identity of the morbid process beyond all objection. Frequent examination of the urinary sediment in both cases will exhibit the similarity of the matters thrown off from the renal tubes, and the identity of the changes proceeding in them; and a comparison of the structural changes in the kidney after death will complete the proof. In both, the kidneys will be beyond the natural weight, large, pale, and exsanguineous, with a few scattered, star-like, vascular points. The cones will present a remarkable contrast of colour to the surrounding deposit, varying from a pale pinkish madder to a deeper tone of madder red; they display the linear markings of the straight tubes, frayed out at the base, fancifully compared to a sheaf of corn.

Microscopically examined, the whole structure of the organ, tubular as well as interstitial, is seen infiltrated with a product which fills and chokes the tubes, obliterates the Malpighian capillaries, permeates the interstitial plexus of vessels, and eventually offers such an obstacle to the course of the blood and circulation through the kidneys as to bring within limits incompatible with life the excretory power of these organs. The mode of death, in both cases, is clearly traceable to this cause; for, in the great majority, the patients die with symptoms referable to a poisonous agency operating upon the nervous centres, and coma, with or without convulsive movements, is usually the indication of the fatal termination by uræmic poisoning.

CASE.—Frances G—, æt. 19, was admitted into Queen Adelaide ward on the 10th of November, suffering from general dropsy of the surface of the body. There was œdema of the face, arms, wrists, and back of the hands, as well as of the trunk and lower extremities. The swollen state of the face usually subsided as the day advanced. There was the usual pallor of the

skin. There was some dyspnoea, with occasional cough and trifling expectoration of catarrhal mucus. The urine had been very scanty, and micturition distressingly frequent: during the night, the desire to pass urine occurred every hour, and the rest was consequently much disturbed. The urine had a cloudy, dirty, soot-like appearance, was highly albuminous, threw down a copious, coarse deposit, and exhibited under the microscope blood-corpuscles, coarse, granular, fibrinous flakes, containing blood-discs; large fibrinous blood-casts, many large granule-cells, and some scattered renal epithelial cells. She complained of a sense of fullness and aching across the lumbar region, and pain was excited by pressure over the region of each kidney. The chest was resonant; some catarrhal wheezing was heard in the large tubes. The sounds of the heart were natural. She stated that four weeks ago she suffered from sore throat, at a time when scarlet fever prevailed in the house in which she lived; that her voice and power of swallowing were affected; but that she had no distinct eruption on the skin; but was told she had, and was treated for, scarlet fever. As near as she can remember, a fortnight after the sore-throat she noticed that her urine became very scanty, and of a dark-red colour, as if it contained blood. The catamenia had not appeared for the last six months. In a few days she became conscious of a swelling in her face after sleep, and subsequently the hands, arms, feet, and ankles become anasarcaous. She was ordered a warm bath daily, the compound jalap powder every morning, a diaphoretic mixture every four hours, and to be clothed in flannel.

On the following day she was dry-cupped across the loins, which, with the above treatment, diminished the sooty appearance of the urine; and in two days the quantity of urine had considerably increased.

A week after admission the dropsy had completely disappeared. The urine was clear, the specific gravity 1018, and albuminous. The deposit, examined by a microscope, consisted of dense fibrinous casts, some blood casts, a few scattered blood-corpuscles; very little epithelium was visible, and no epithelial casts could be seen. She was ordered the sesquichloride of iron, and an improved diet. But this treatment appeared to have been commenced too early, for on the 25th of November there was a return of hæmaturia, which, however, disappeared by the loss of a few ounces of blood from the loins by cupping.

On November 30th she again began to improve; there was no return of œdema, although the urine continued highly albuminous.

On December 4th the urinary sediment first gave warning of the unfavorable change which was commencing in the cell-structure of the tubes, notwithstanding the disappearance for a time of the dropsy, and the apparent improvement in the general condition of the patient. The urinary sediment had been examined every two or three days since admission, and up to this date it consisted of fibrinous casts, large, coarse granular casts, containing a few blood corpuscles and a few scattered epithelial cells; but no epithelial casts, or free shedding of the epithelium, which is often observed in the dropsy after scarlet fever. But now the casts were becoming transparent, containing large compound granule-cells, some exudation-corpuscles approaching in character to the pus-cells, and a few epithelial cells; and some of the casts contained numerous resplendent granules—the disengaged nuclei of disintegrated epithelial cells. Two days later the casts became still more transparent or hyaline, with an increased number of exudative corpuscles with reniform and trefoiled nuclei. The epithelial cells did not present any characteristics of fatty or abortive development. On the 11th of December, the anasarca of the surface again made its appearance. The urine became

less abundant. Purgatives were again beneficially employed. The sesquichloride of iron was again taken, and for a few days another interval of improvement, so far as the diminution of the anasarca was accepted as a sign, was apparent. But the urinary sediment became more abundant. Large, solid moulds of fibrine, the greater part containing no cell-structure, many hyaline-moulds, containing large, compound granule-cells, and many free nuclei, indicated that the disease was progressing most unfavorably. On the evening of the 27th of December, she became very restless; and on the morning of the 28th, suddenly calling for assistance, she almost instantly became insensible. The jaws became locked; there was some rigidity of the extremities; the pupils were dilated; but there was neither convulsive movement, nor stertor, nor foaming at the mouth. The respiratory movements gradually became slower and less apparent, and she ceased to breathe, without any movement or spasm, at eleven a.m.

Post-mortem examination twenty-eight hours after death.—The external surface of the body was but slightly anasarcaous. The pleural and abdominal cavities contained a small quantity of orange-coloured serum. The lungs were free from pleuritic adhesions, but were slightly œdematous. The heart was large; it weighed thirteen ounces and a half; there were several maculæ albidæ, both on its anterior and posterior surfaces; the cavities of the heart, as well as the valves, were natural. The liver was healthy. The left kidney was the larger of the two; it weighed seven ounces and a half; it was irregular in shape, considerably lobulated above and below, very convex anteriorly, somewhat flattened posteriorly. The capsule was semi-adherent, and when removed, the kidney displayed a finely granular (sabulous) surface, pale in colour, with a few stellar vessels scattered over it. The interior, on a section being made, displayed the usual fine granular exudation infiltrating the whole structure, fraying out the base of the cones, and contrasting in colour with the pale-red striated appearance of this pyramidal portion of the organ. The right kidney weighed five ounces and three-quarters; the cones were more deeply injected than the other kidney, but in other respects the morbid conditions were similar, the disease not having advanced to the same point, one kidney being usually behind the other in progress.

A careful microscopic examination of these kidneys was made. The white deposit surrounding the cones, and constituting the mass of the disease, was composed of abortive cells, embedded in what appeared to be a fine granular exudation. This granular matter pervaded the whole organ, and was principally interstitial in regard to its relation to the tubes, as but little tubular structure could be discovered in it. The Malpighian bodies, however, appeared stuffed with this exudation, and the few tubes which were teased out of the deposit, appeared blocked up with it. The epithelial cells along the walls of the tubes, at the base of the cones, appeared undisturbed, but compressed by the contents of the canal. The straight tubes at the apex of the cones were free open canals, the epithelium presenting a natural appearance; but here and there a large straight tube was observed choked with fibrinous coagula, of a brownish-yellow colour.

ART. 61.—*On the absence of Odorous Principles in the Urine in Bright's Disease.* By Dr. DE BEAUVAIS.

(*Comptes Rendus*, Oct. 25, 1858.)

Dr. de Beauvais looks upon an inodorous condition of the urine as a constant accompaniment of Bright's disease. Clinical inquiries, often

repeated, he tells us, have convinced him that this condition of the urine is pathognomic of this disease, that it continues even when the symptom of albuminuria is temporarily absent, and that it never accompanies those forms of albuminuria which are not dependent upon Bright's disease. Dr. de Beauvais adds, also, that this inodorous condition of the urine is not changed by the ingestion of turpentine, asparagus, or any other substance, the action of which, under ordinary circumstances, is to communicate a strong and peculiar odour to the urine.

ART. 62.—*On the use of Tannin in Albuminous Anasarca.*

By Dr. P. GARNIER.

(*Archiv. Gen. de Med.*, Jan., 1859.)

In this paper Dr. Garnier gives nine cases, for the purpose of showing that tannin, in daily doses of thirty to sixty grains, is of great use in relieving or curing the passive œdema or anasarca which accompanies albuminous urine. The change for the better is not at all deferred, and in two or three days the urine becomes more abundant and natural, the perspiration more copious, the stools less costive, the appetite better, and so on. The evidence, however, does not appear to warrant all these favorable conclusions.

ART. 63.—*On the use of Iodine in Diabetes.*

By Dr. DICK, Professor of Medicine in the Edinburgh Veterinary College.

(*Edin. Med. Journal*, Nov., 1858.)

In a notice of Dr. Stokvis's researches on the formation of sugar in the liver, it is mentioned incidentally that Professor Dick has found iodine in large doses, ʒj. twice or thrice a day, a perfect cure for diabetes in the horse. It is suggested, also, that this remedy might be serviceable in the saccharine diabetes of man, and the suggestion is certainly one which deserves to be attended to, for iodine has certainly a marked action on the organ which is a good deal concerned in the production of the sugar, namely, the liver.

ART. 64.—*Sugar in the Tears in Diabetes.* By Dr. GIBB.

(*Proc. of the Path. Society*, May 4, 1858.)

CASE.—On the 20th April, 1858, Dr. Gibb examined some tears in a small tube, shed by Mrs. W—, a lady, æt. 21, who has been affected with diabetes mellitus for two years, ever since she weaned her only child. A drop or two of Barreswill's solution of copper were added, the fluid was boiled over a spirit lamp, and at once gave an abundant precipitate of the yellow sub-oxide of copper. The same experiment with an equal bulk of the urine, showed that there was a much larger proportion of sugar present in the tears. The specific gravity of the urine was 1043. A drop of each fluid was evaporated on a piece of glass, when that from the tears showed a much thicker and more opaque crust than was yielded by the urine. On the 9th

of April, when she first came under my care, her urine was passed in quantities, varying from twelve to fifteen pints a day, and she drank at least eight pints of fluid in the twenty-four hours. Now, the quantity is perfectly natural, her thirst is diminished, and she takes her usual allowance of food and drink. The urine is still highly saccharine.

ART. 65.—*Notes on Pigment in the Urine.*
By Dr. A. J. PAINE, Bengal Medical Service.

(*Indian Annals of Med. Science*, July 1885.)

Dr. Paine relates several cases which appear to show that when the function of the liver is not duly discharged, whether the organ be itself primarily affected, or whether it be involved consecutively in some more general disorder, an increased proportion of pigment in the urine will generally serve us as evidence of the fact; and that this increase, when present, may safely be taken as a positive indication of hepatic disturbance, even though there be no other symptom to confirm it; and, negatively, as a necessary consequence of the first proposition, and with the same exceptions to be specified hereafter, if the state of the urine be normal, it affords very strong presumption that the liver is healthy.

“The state of the liver indicated by this pigmented urine is no doubt an incipient stage of that which, further developed, produces jaundice from arrest of biliary secretion. It is a comparatively rare occurrence that this latter stage is reached, though it happens now and then before our eyes that the lesser is the precursor of the greater change. When it is remembered that extreme jaundice occasionally takes place without giving rise to any alarming symptoms, it need create but little surprise that the same condition, less developed, should commonly escape detection—in other words, that, in patients in whom no symptoms have occurred to direct attention to the liver, the urine, when examined, should be found to give evidence of imperfect discharge of its function. It must not, however, be inferred that a highly pigmented urine always expresses a state of systemic disorder, or indicates that specific treatment is required. Many instances undoubtedly occur where marks of former disease of the liver exist, although the patient has to all intents and purposes recovered. Without being sufficient to impair the general health, the change will show itself in the state of the urine, as a transfer of function from the liver to the kidney; and, if this be held to prove that recovery from former attacks is incomplete, and treatment be again directed to the liver, there is danger of lighting up fresh disease in that organ. It appears that, in these cases, though the liver has not been restored to health throughout its whole volume, yet, physiologically speaking, recovery has been complete, diseased action has ceased to go on, the remainder of the gland works well, and a compensatory process has been set up in the kidney, whereby an abnormal quantity of highly carbonized matter is eliminated from the blood, and the failure of a portion of the liver is repaired. It is further probable that, if this vicarious excretion were to cease, the symptoms of inadequate liver would present themselves; as is seen in cases of jaundice, where the

disappearance of pigment from the urine becomes a sign of very serious import.

"There are few who, when examining the bodies of persons dead of various diseases, have not found marks of former lesions in the liver, fissures and cicatrices of different shapes. These had given no evidence of their presence during life, but it is probable that excess of carbonized matter would have been found in the urine, if it had been sought for.

"Further, the quantity of pigment in the urine is no index of the severity of disease; that is, it is not a measure of the permanent effect to be apprehended. It is very small in some fatal cases of abscess and very large in others where there is only congestion. It is extent, not intensity of disease, and interruption of proper function, that are measured; the effect in fact on the purification of the blood, but on no other vital process.

"It is scarcely necessary, after the foregoing remarks, to point out that this sign will not assist us in diagnosing the several diseases of the liver from each other. This must be done by consideration of the conditions under which the disease has arisen. The pigments tell us that something is wrong, that there is an omission on the part of the liver. They do not indicate what is going on instead of its proper function, but simply that the latter is in abeyance, and they gauge the degree to which it is suspended. It is possible, however, that the difference discoverable by chemistry between the peculiar pigment of the urine, and that of the bile in urine may, ultimately, serve us as a means of diagnosing jaundice, the result of suppressed secretion, from that which follows on closure of the ducts. Dr. Budd anticipates that some such diagnostic sign will be found in the urine, and inclines to the supposition that oxalate of lime may afford assistance: but, since it has been shown by Dr. Owen Rees that the latter product is commonly found under circumstances which determine a deposit of lithates, we may expect to see it under all conditions which embarrass the portal circulation, and cannot hope to use it as diagnostic of any peculiar affection. At the same time, it is possible that the oxalate may be in closer relation with the highly carbonized pigment than is generally supposed, an idea suggested to me by the fact that—while, as in Dr. Rees' experiments, the characteristic crystals appear readily on heating the coloured deposit of lithates—no such result follows with the pale variety."

The phenomena presented by pigmented urine, and the mode of examination are also fully gone into by Dr. Paine, and his remarks upon these subjects are well deserving of attention.

(F) CONCERNING THE CUTANEOUS SYSTEM.

ART. 66.—*On the nature, causes, statistics, and treatment of Erysipelas.*
By Mr. PETER HINCKES BIRD.

(Pamphlet, Churchill, pp. 60, 1858.)

The design of this essay is to establish that the distinctive characteristic of erysipelas, and the diseases allied to it, is their origin in a

morbid poison in immediate connexion with an unhealthy condition of the system. A subtle poisonous matter enters the blood, constituting, in fact, a true blood-poison disease; and the almost universal rule, that the presence of a poison in the blood is productive of a direct and special operation on the skin, follows, in this particular instance causing erysipelas.

But what Mr. Bird particularly wishes to insist upon is, that the characteristics of this diffuse inflammation may be manifested in other tissues than the skin or subcutaneous cellular membrane, and that there is no conclusive reason why this species of inflammation should be confined to the external surface of the body; indeed, many acute and experienced observers have recorded instances without number, of internal parts presenting every specific mark of that diffuse inflammation which, when confined to the skin, is termed erysipelas. Some accidental cause may determine in which tissue it shall appear. In a person predisposed to this form of inflammation, it may become developed wherever the immediate excitement may be thrown. Thus, if it be a wound, erysipelas will appear; if a sloughing bubo, strangulated hernia, or recent delivery, diffuse inflammation of the peritonæum will result; if a punctured vein, diffuse phlebitis, &c.

Some writers go so far as to include all diffuse inflammations under the word erysipelas, but Mr. Bird doubts the advantages of stretching the application of the term so far, and thinks the term "diffuse inflammation" is wide enough to include the facts and liable to no misconception or obscurity; agreeing with Hildenbrand, who justly observes—"Absente dermatitis typo, etiam *erysipelatis* characterem amittunt."

These alliances of erysipelas may be thus grouped together :

1. Diffuse inflammation of the cellular tissue.
2. Diffuse inflammation of the mucous membranes.
3. Diffuse inflammation of the serous membranes.

1. *Diffuse inflammation of the Cellular Tissue.*

There is much evidence in favour of the opinion of the close affinity between erysipelas and diffuse inflammation of the cellular tissue. The general and many of the local symptoms of diffuse inflammation of the cellular tissue closely resemble those of erysipelas. The commencement of the disease is marked by the same general derangement, and the constitutional symptoms are of the same character; the changes locally produced by erysipelas in its cellulo-cutaneous form and diffuse cellular inflammation are wonderfully alike: the predisposing causes are the same, the results identical if resolution does not take place. Diffuse cellular inflammation occurs, in connexion with puerperal fever, a disease which, as will be afterwards shown, is closely allied to erysipelas. Diffuse cellular inflammation and erysipelas prevail epidemically at the same time. One form of the disease is extremely liable to produce the other in a second person; and the two forms of disease are mutually capable of exciting each other.

Many circumstances connected with the coincident appearances and causes—the circumstances under which it is most rife, its mode of

propagation, its accompanying local and constitutional phenomena, the prevalence of the two diseases at the same time—induce a strong suspicion of a close alliance between hospital gangrene, malignant pustule, and erysipelas.

2. *Diffuse inflammation of the Mucous Membranes.*

Numerous writers refer to the close affinity between erysipelas and diffuse inflammation of the mucous membranes.

There is strong evidence in favour of the opinion that emanations from those with erysipelas will produce diffuse inflammation of these membranes; and Dr. Bird has met with several cases in which a direct extension of the diffuse inflammation had manifested itself from the fauces to the external skin by the anterior nares and lachrymal passages, and *vice versâ*; and the extension of the diffuse inflammation down the throat has been observed to appear around the wound of tracheotomy in the form of erysipelas.

A diffuse inflammatory state of the fauces has been mentioned as a precursor of erysipelas; and this is so frequently the case that it was observed in nearly 60 per cent. of the cases of idiopathic erysipelas of the face of which the author has taken notes, and is therefore included in the definition of the term; it was sometimes observed in idiopathic, and occasionally, but much more rarely, in traumatic erysipelas of the extremities. How is this more frequent occurrence in idiopathic than in traumatic erysipelas to be accounted for? Is it that the miasm in being inhaled into the lungs makes a direct impression on the throat, while in its entrance into the circulation by means of a wound this mode of contact is avoided?

Some interesting cases have been witnessed of erysipelas spreading down the throat, and also of extending up the vagina and rectum.

The author remembers the case of a woman who, after rigors, had diffuse inflammation of the throat, which spread upwards from the nostrils and mouth to the face; and from the violent retching and distressing sickness, excessive flatulence and relaxed bowels, it seemed probable that it even spread down the alimentary canal to some extent. Wine, which was the only thing that could be retained on the stomach, was liberally given, and she recovered.

Diffuse inflammation of the mucous membrane is not unfrequent in those attending on cases of puerperal fever: the value of this fact will be appreciated when the alliance between erysipelas and puerperal fever is determined.

There are occasionally observed, in the surgical wards of hospitals, other cases which seem to point out clearly the alliances of erysipelas; thus, supposing there are several cases of erysipelas of the head, face, or extremities, patients who have taken neither mercury nor iodine become subject to acute stomatitis, others have swelling and tenderness of the tongue, accompanied with diffuse inflammation of the fauces, and considerable constitutional disturbance.

From the occurrence of these cases of cynanche with ordinary erysipelas, from the fact that we can often trace the continuity of inflammation from the fauces to the face, through one or several of the passages

by which the mucous membrane is continuous with the skin, and *vice versa*, from the connexion of these with occasional glossitis and stomatitis, from the condition of the mucous membrane accompanying these affections, and from the general testimony in favour of its contagiousness, we can but conclude that these affections are instances of these structures being attacked by diffuse inflammation, which when limited to the skin is properly termed erysipelas.

3. *Diffuse inflammation of Serous Membranes.*

Various writers have included these affections under the term "erysipelas," and the evidence to be produced will serve to prove the strong alliance existing between them.

(a) *Diffuse arachnitis.*—It is difficult to say how far diffuse arachnitis is allied to erysipelas; but it is pretty certain that in many cases of erysipelas of the head this alliance can be traced. Alibert evidently inclines to the opinion that the membranes of the brain may be affected by diffuse inflammation in connexion with erysipelas. Nunneley believes that the frequent purulent deposits in various viscera will never or rarely be found to follow accidents to the head unless preceded by diffuse arachnitis; but in patients who have died from erysipelas of the head the author has by no means found this condition of frequent occurrence.

(b) *Diffuse inflammation of the peritoneum, pleurae, pericardium, &c.*—Diffuse inflammation of these membranes has been frequently noticed supervening upon erysipelas.

(c) *Puerperal fever.*—Writers even from an early period suggested, and the limited testimony of later authors confirm, the connexion which exists between erysipelas and puerperal fever. It was first observed and insisted upon by Pouteau, in 1750, who considered the puerperal fever as it then prevailed in Paris as an epidemic erysipelas of the peritoneum, and others subsequently maintained this opinion.

The following is striking evidence in favour of this opinion. The local symptoms during life and the appearances after death are identical, allowance being made for the different situation and texture of the parts attacked; both puerperal fever and erysipelas arise under the same circumstances, in crowded and close wards; both diseases are marked by great disposition to the formation of unhealthy pus; the same danger attends inoculation with the fluids effused in puerperal fever, and the immediate development of erysipelas or malignant pustule in the part inoculated; both erysipelas and puerperal fever may exist in the same patient; they prevail at the same time; and may during life mutually produce each other in a second person.

(d) *Diffuse inflammation of veins, arteries, and lymphatics.*—Diffuse phlebitis. The alliance between these affections and erysipelas may at first glance appear slight, but many circumstances tend to point out a closer connexion.

So similar are the constitutional and even the local symptoms of diffuse phlebitis and erysipelas, that even the best observers have made errors in diagnosis. In both these diseases there is the same disposition to the deposition of pus in different organs, and for inflammation and imperfect suppuration to be set up in distant parts of the body; there is the same

tendency for the serous membranes to become affected; both diseases depend upon constitutional causes, more than upon the immediate exciting cause; one form of complaint is very liable to produce the other; and it has been observed that the same condition of atmosphere conduces to erysipelas and diffuse phlebitis. Abundant evidence has already been brought forward to substantiate the opinion, that puerperal fever prevails most when erysipelas and other diffuse inflammations are rife—produces them, and is produced by them; and as one form of puerperal fever consists in diffuse inflammation of the uterine veins, much of the evidence in proof of the close alliance between erysipelas and puerperal fever is applicable here.

Diffuse inflammation of arteries. It is probable that, like other serous membranes, the internal structure of arteries may be subject to diffuse inflammation, in connexion with erysipelas, but Mr. Bird has not met with any instances. Guthrie speaks of erysipelatous inflammation of arteries, and gives three instances.

Diffuse inflammation of lymphatics. As the lymphatics, owing to their general and dense distribution within as well as between all the organs of the body, necessarily take part in every inflammation, they are no exception to the general rule.

The two diseases are occasionally observed to be complicated to a remarkable extent; many patients affected with angioleucitis being soon after attacked with cutaneous or cellulo-cutaneous erysipelas, and *vice versa*, those who were first attacked with erysipelas soon presenting it complicated with angioleucitis.

The following analysis of 260 cases of erysipelas, of which Mr. Bird has notes, may prove interesting:

	Men.	Women.	Total.
Idiopathic of face, head, &c.	34	51	85
Traumatic of ditto	27	13	40
Idiopathic of extremities	27	22	49
Traumatic of ditto	59	27	86

260

	Rigors and constitutional symptoms were observed in	Constitutional symptoms without rigors in	Precursory sore throat.	No precursory symptoms.
Idiopathic of face	Men 23 or 67·6 per c. Women 38 74·5 "	16 or 17·6 per c. 7 13·7 "	14 or 41·2 per c. 31 60·7 "	4 or 11·8 per c. 3 5·8 "
Traumatic of face	Men 8 29·6 " Women 6 46·2 "	10 37·0 " 5 38·5 "	2 7·4 " 1 7·6 "	9 33·3 " 2 15·4 "
Idiopathic of extremities	Men 12 44·4 " Women 13 59·1 "	10 37·0 " 4 18·2 "	3 11·1 " 3 13·6 "	2 7·4 " 1 4·5 "
Traumatic of extremities	Men 25 42·4 " Women 11 40·7 "	15 25·4 " 10 37·0 "	2 3·4 " "	17 28·8 " 6 22·2 "

Of forty-nine cases of idiopathic erysipelas of the face in women, and of thirty-two in men, it was observed that the disease attacked the different parts of the face in the following order of frequency:

	Women.	Men.
Right side of face	29 or 56·9 per cent.	17 or 50·0 per cent.
Left side of face	10 19·6 "	10 29·4 "
Middle line of face	7 13·7 "	4 11·8 "
Both sides at same time	3 5·9 "	1 2·9 "

In these 260 cases, twenty deaths occurred, or 7·5 per cent. Of these, one case was complicated with severe burn, one with dropsy after scarlet fever, one with advanced phlebitis, one with a severe scalp wound,

two were in articulo mortis when seized, another was dying from extreme neglect before admission.

These 260 cases occurred in the seasons in the following order :

Spring, 66. Summer, 49. Autumn, 56. Winter, 89.

Thus it would appear that erysipelas is more frequent in winter and spring ; and that it is more fatal in the metropolis during these seasons, the following extracts from the returns of the Registrar-General will prove :

	1845.	1846.	1847.	1848.	1849.	1850.	1851.	1852.	1853.	1854.	Total.
Spring -	95	71	116	196	137	119	81	120	86	96	1117
Summer -	80	78	107	129	114	103	74	98	74	115	972
Autumn -	56	92	126	128	99	65	76	54	80	109	875
Winter -	77	106	176	126	109	87	116	67	84	128	1076

As to treatment, Mr. Bird concludes that erysipelas is best treated by stimulants and support, and, when complicated with inflammation of the subcutaneous cellular tissue, by early incisions, which should extend to the depth of the tissues.

The paper concludes with an elaborate analysis of the work of Fenger—‘*De Erysipelate Ambulanti Disquisitio.*’

ART. 67.—*On Macula Cachectica.* By Dr. JAMES WHITEHEAD.

(*Third Report of the Clinical Hospital, Manchester, 1859.*)

“A maculated state of the skin,” says Dr. Whitehead, “is frequently met with in the children of this class of people, consisting of small circular spots the size of face freckles ; not, however, occupying the face and hands as freckles do, but every other part of the body except the feet. They seem to infest parts covered by the clothing, while freckles are only seen on parts exposed. They have a purple colour, like the maculæ of typhus—not a pale yellow like freckles, and are unassociated with any form of disease saliently expressed ; but they seem, nevertheless, to indicate a depraved habit of body. The skin has a sickly, opaque pallor, the flesh is flabby, the temper fretful, and the energy subdued. The parents appear to regard their presence as natural and unavoidable, as they are never the subject of treatment, nor is attention directed to them when the body is examined for other purposes. They seem peculiar to the lowest classes, and are caused by sloth and personal neglect, as they are never met within the offspring of the cleanly and thrifty.

“There is no doubt that this *macula cachectica* is in reality a disease of the skin, due, proximately, to interrupted cutaneous transpiration. Its cause is uncleanness. Such children are seldom washed, except the hands and face, oftener than once in many weeks, and even then very imperfectly, and without the use of soap. When the children get older, and the washing process is left to themselves, it is still less perfectly done, and the whole body is not washed, in the vast majority of instances, once in several years. In such persons the spots which appeared in infancy, are often seen up to adult life.”

ART. 68.—*Case of Chromidrosis.* By Dr. MAKER, of Colmar.

(Gaz. des Hôpitaux, No. 6, 1858.)

CASE.—Marie V—, of Wettolnheim, near Colmar, æt. 19, a blond, having a white skin, and always delicate in constitution. She began to menstruate at 13, but menstruation was never established regularly. Three years ago she suffered for several months from amenorrhœa. Twice in 1855 her face exhibited for some weeks a decidedly bluish colour. This bluish colour returned in March, 1858, and became more and more marked until the time when Dr. Maker saw her (26th Sept. 1858), when at first sight her face, neck, arms, and hands, seemed all over bruises. The colour, which was deepest about the eyes, was of a full indigo blue. On rubbing the skin with an oiled linen cloth, the blueness might be completely wiped away, and the skin restored to its natural condition; on examining with a lens, the colouring matter was seen as a fine dust deposited in the cutaneous crevices. The colour became much deeper during exercise or under the warm rays of the sun; it was also somewhat deeper in the morning than at a later period of the day. Occasionally, also, the saliva had a decided blueish tint. As to the rest,—the patient was dyspeptic and anæmic, and for some time she had suffered from profuse leucorrhœa. An analysis of the colouring matter showed that it was analogous, if not identical with indigo.

ART. 69.—*On the occurrence of Bots in the human subject.* By (1) Dr. G. W. SPENCE, of Lerwick, Shetland, and (2) Dr. J. MATTHEWS DUNCAN, of Edinburgh.

(Edin. Med. Journal, Nov., 1858; Edin. Vet. Review, Jan., 1859; and Medico-Chir. Review, April, 1859.)

Alexander von Humboldt so frequently found the natives of South America affected with a number of the genus *Estrus*, an insect of the order Diptera, that he established a distinct species, under the name of *Estrus humanus*. Kirby and Spence, Kirchenmeister and others, dispute the propriety of this distinction, and Doctor Spence and Dr. Duncan bring forward fresh arguments and facts as well as cases that fell under their own observation, in proof of the view that *Estrus humanus* is merely a transference of one of the varieties of the bots found on animals to the human body. In our October number (1858), we quoted from the 'Archives Générales' an interesting account by Dr. Coquerel of the development of the larvæ of a Diptera in the frontal sinuses and nasal fossæ of man. In Dr. Duncan's case, the symptoms were less severe; it is as follows:

E. C—, a girl, æt. 13, came from Perthshire in September, 1853, to reside in Edinburgh. She had never been in bad health till shortly after leaving the country, when she began to suffer pains which she connected with the bots. She first felt a little lump on the back of the neck, which slowly changed its position in various directions; then a hole opened over it, and a worm was squeezed out. Some weeks afterwards, another similar lump was felt on the right side of the trunk. It also wandered about subcutaneously, till a hole opened over it, and it was forcibly rubbed out. A third made its appearance over the spine,

high in the chest, then travelled up the neck, when for a time it was lost, and was supposed to reappear on the right side of the neck, where a hole formed over it. Dr. Duncan was now (3d March) fortunate enough to be called, and observed a small, not inflamed tumour, of the size of a large field-pea, and having an opening on the top as big as a pin's head. In this hole Dr. Duncan saw one or two black points rolling about. On squeezing moderately the little lump, there was discharged a living larva half an inch long, evidently of the *Æstrus bovis*, and the same as those previously noticed by the girl. A little dirty-yellow juice issued with the animal, containing a few blood-globules and pus-corpuscles. The girl said that while in Perthshire herding cows, she was much exposed to the air, and was frequently stung by insects.

It appears from Dr. Spence's paper, that in the Shetland Isles the human subject is peculiarly subject to being attacked by the skin-bots; the larvæ occur in exposed parts of the body, and in women who are loosely dressed. They have generally been engaged during the summer in working with peats, in localities where cattle were numerous; in some instances they had lain down on the grass and fallen asleep. Dr. Spence's patients never remembered having felt a sting; they were engaged in the only occupation by which people in Shetland are liable to be detained in localities where the gad-fly or bot-fly may be met with.

The patient's attention, according to Dr. Spence, is first attracted by a severe burning pain under the skin, in a circumscribed spot. It is next observed that the seat of pain shifts its position, and that the course between the two spots is marked by a reddish or ecchymosed line, which fades in a few days. This movement of the seat of pain, attended with a thin linear discoloration of the skin marking its track, may be almost considered as a diagnostic sign. The rate of progress varies; in one case, Dr. Spence traced it between the 29th September to the 10th October, from the left hip to above the left mamma. The larvæ appear to drop out after a time of their own accord, but the safest and most expeditious plan is, as soon as their nidus is ascertained, to cut down upon and remove them.

ART. 70.—On "*Sapo Laricis*" in cutaneous diseases. By Dr. MOORE.
(*Dublin Hospital Gazette*, March 15, 1859.)

Dr. Moore having referred to some cases of subacute and chronic affections of the skin, in which he found the internal and local exhibition of larch bark very efficacious, alluded to the difficulty of applying the ointment in cases of extensive psoriasis, ichthyosis, or similar forms of disease, in which entire extremities, or even the whole body, may be implicated; accordingly the preparation of the following soap suggested itself to him as being equally effective and easier of application:

Take Wheaten bran, ℥iv;
White curd soap, ℥xxiv;
Pure glycerine, ℥iii;
Extract of larch bark, ℥vi;*
Rose water, ℥xii.

* The proportion of the extract may be increased or diminished to suit the severity of the case, or otherwise.

The twenty-four ounces of white curd soap were dissolved in twelve ounces of rose water, by means of a steam bath; the four ounces of wheaten bran were infused in ten ounces of cold water for twenty-four hours and expressed; to the expressed liquor the three ounces of pure glycerine* were added; the six drachms of the extract of larch bark were dissolved in one ounce of boiling water; these solutions when mixed were added to the dissolved soap. The superfluous water was evaporated by means of a steam bath, until the soap became of a proper consistence for pouring into moulds. The product is a soap of a dark claret colour, with rather an agreeable smell.

Dr. Moore found this soap very efficacious in the local treatment of psoriasis, pytiriasis, chronic herpetic, and eczematous affections; it may be applied once or twice daily, according to circumstances, and allowed to dry on the part; when the necessary ablutions are required, lotions of bran-water, or pure water with carb. soda (℥ss to ʒj to Oj) will be found usually most suitable; occasionally more stimulating lotions may be indicated.

* Besides the lubricating properties of the glycerine, chemically, its solvent action on the tannin in the larch bark is of great importance.

PART II.—SURGERY.

SECT. I.—GENERAL QUESTIONS IN SURGERY.

(A) CONCERNING INFLAMMATION.

ART. 71.—*The treatment of Burns by constant warm baths.*
By Dr. PASSAVANT.

(*Deutsche Klinik*, Nos. 36, 38, and 39, 1858.)

DR. PASSAVANT'S experiments were made in the hospital at Frankfort upon eighteen persons who had been all more or less seriously burnt in the explosion of a firework manufactory. The apparatus used was analogous to that used by Langenbeck in the treatment of amputations: the water, which was changed twice a day, or oftener if the suppuration was abundant, was kept at 27° Reaumur; and at the end of some weeks, when the patients had become tired of them, the baths were changed for fomentations. The results are represented as being very satisfactory. Under their use pain and inflammation very soon came to an end. The hardened tissues became soft, and the eschars separated readily. The chances of irritation and purulent absorption became greatly diminished; and cicatrization proceeds more rapidly.

(B) CONCERNING TUMOURS.

ART. 72.—*A contribution to the statistics of Cancer, collected from the records of the Middlesex Hospital.* By Mr. SIBLEY.

(*Proc. of R. Med.-Chir. Soc.*, March 8, 1859.)

This paper is the result of an examination of 519 cases of cancer, with the records of 172 *post-mortem* examinations. The more recent cases have been reported with uniformity and with some degree of fulness; some of the older cases were less perfect. The diseases embraced within the limits of the paper are defined, and distinguished from those which had been excluded from consideration. A table is then given, in which the

seat of the primary cancer in each of the 519 cases is exhibited. There are 103 instances in the male, and 416 in the female; amongst the latter are 191 cases of cancer of the breast, and 156 of uterine cancer.

There are three examples under the age of ten (all males), and one between the age of ten and twenty. Tables are given, in which the ages are arranged in decennial periods, the cases of breast and of uterine cancer being placed in separate groups. The average age of those attacked with uterine cancer is 43·28 years; with breast cancer, 48·6.

Of the female cancer patients, 83 per cent. either are or had been married, and amongst the single women the disease occurs oftener in the breast than in the uterus. Of the married women, 86 per cent. of the uterine, and 74 per cent. of the patients with breast cancer, have borne children. The average number of the births is 5·2 among the former, and 3·89 among the latter. The interval between the last pregnancy, and the proportion attacked before and after the cessation of the catamenia, are given.

The duration of life (from the first discovery of the disease), in patients who had not been operated on, varies greatly in the different classes of cases. In the breast it is $32\frac{1}{4}$ months; in the uterus, 14; in the stomach, $8\frac{1}{2}$; in the rectum, 34; in the lip, face, &c., 53; in the penis, $34\frac{1}{2}$; in the bones, 10; in the labium, 29. These figures are not perfectly comparable, as in some cases, especially the external cancers, the period given is the entire duration of the disease, while in others (as in the stomach) the period is only that during which the symptoms were present.

An account is then given of the operations (by the knife) in cases of cancer of the breast. Three patients out of 60 died from the effects of the operation. The average duration of life of those who were operated on is 53·2 months. In comparing this with the duration of life in cases in which the disease was allowed to run its natural course (32·25 months), it should be remembered that the cases submitted to operation are more or less selected cases.

As to the hereditary nature of the affection, the difficulties in obtaining accurate information upon this point are first alluded to. The chief of these is the very imperfect knowledge which most people, but more especially hospital patients, possess of the diseases to which their relatives had been subject. Out of 305 cases, in which the point had been particularly inquired into, 34 patients remembered to have had a relation affected with cancer. A table is given of the seat of the disease in each of the 34 cases, in 17 of which the breast was the part affected. Tables are also given, in which the degree of relationship of the cancerous relative is shown, and also the proportion affected on the father's and on the mother's side. Out of the 34 cases, in six more than one relative was cancerous, and in one instance (the chief features of which are mentioned) no fewer than five relatives suffered from cancer.

Phthisis existed in 50 cancerous families out of 130. Similar tables to those before mentioned were given, in which the degree of kinship was exhibited; it being also noted whether the disease was on the father's or the mother's side.

The notes of the 172 *post-mortem* examinations are next analysed. In the first place, a table is given, in which the seat of the primary cancer in each instance is exhibited. The cases are arranged in the following

groups: 1. Cancer of the breast. 2. Cancer of the uterus. 3. True cancer of other organs. 4. Epithelial cancer. A series of tables follows, in which the secondary cancers are enumerated, and the cases arranged as follows: *a.* The disease strictly local. *b.* Involving also the lymphatics of the part. *c.* Involving the lungs and other parts, the liver being unaffected. *d.* The liver cancerous, the lungs being free from this disease. *e.* Those cases in which there were tumours in distant parts of the body, but both the lungs and liver were free from the disease. In each form of the affection, a list of the non-cancerous diseases found in the bodies of the cancer patients was appended.

The bearing of the foregoing facts on the mode in which cancer is disseminated throughout the body is next alluded to, three distinct modes of multiplication being recognised: 1st, the growth of tumours in the immediate neighbourhood of the cancer; 2d, the development of cancer in the lymphatics of the part; 3d, the formation of cancerous tumours in distant parts of the body.

In regard to the cachexia, it was noticed that this condition only became developed as the ulceration and sloughing extended, and could not be attributed to pre-existing changes in the condition of the blood of the patient. In nearly all instances the patient died from the ordinary effects of ulceration, or from the interference with vital functions.

(C) CONCERNING WOUNDS AND ULCERS.

ART. 73.—*On the treatment of Wounds and Ulcers by Ventilation.*
By Dr. J. BONISSON, of Montpellier.

(*Comptes Rendus*, Oct. 4, 1858.)

By means of an ordinary bellows, or by some special contrivance for producing a draught of air, Dr. Bonisson dries up the effused fluid, and obtains, in this manner, a crust by which the wounds or ulcers are covered and protected. The process of healing, he tells us, advances more favorably, and more rapidly, under these circumstances—*sub-crustean* cicatrization being for open wounds what *subcutaneous* cicatrization is for closed wounds. One of the advantages of this plan is the saving which it effects in charpie and other dressings.

(D) CONCERNING FRACTURES AND DISLOCATIONS.

ART. 74.—*Additions to the statistics of Fractures.*
By Dr. O. HEYFELDER, of Munich.

(*Dublin Quarterly Journal of Medicine*, Nov., 1858.)

Dr. O. Heyfelder's individual calculations are made upon 562 fractures which were admitted into the University Hospital at Erlangen, between 1852 and 1854. To these calculations are added those of Middeldorf and Malgaigne; and the mean frequency of breaking, as

regards the different bones, is as follows:—Both bones of leg, 1 in 7; femur, 1 in 7; ribs, 1 in 9, and so on.

Both bones of legs	1	Tibia	1
Femur	7	Patella	26
Ribs	9	Metacarpus	68
Fore-arm	10	Mandibula	84
Radius	11	Olecranon	92
Clavicle	13	Vertebræ	126
Humerus	17	Bones of the toes	133
Cranium	19	Pelvis	170
Fibula	25	Scapula	202
Bones of the face	28	Tarsus	230
Bones of the fingers	32	Metatarsus	300
Ulna	41	Sternum	400
			440

ART. 75.—*On the treatment of Sprain by Friction and Shampooing.*

By M. GIRARD.

(*Mon. des Hopitaux*, No. 140; and *Medico-Chir. Review*, Jan., 1859.)

In this paper, laid before the “Académie de Médecine,” M. Girard states that his attention was first directed to the plan of treatment he describes by the manipulations of an empiric. So successful were these in a bad case of sprain, that he determined to investigate the subject, and as the result of numerous trials, he now proposes what he believes to be a very effectual and a rapid procedure, for the treatment of what too often proves a very tedious and serious affection.

No matter what the severity of the sprain may be, its treatment should be commenced by the gentlest friction, the points of the fingers scarcely touching the skin. After practising such frictions from below upwards for from ten to twenty minutes, it will be almost always found that a certain amount of pressure can be borne, and this is to be increased or diminished according to the sensations of the patient.

It is very rare that we can proceed in this manner for half an hour without the patient declaring that his pain is notably relieved. Arrived at this point, when the patient can bear the weight of the hand, we proceed to the shampooing. This is performed not only with the fingers (which, kept close together during the frictions, are now to be separated, so as to pass into the various sinuosities of the part), but also with the palm of the hand, so as to embrace the entire joint and surrounding parts. The hand in both this and the former part of the procedure should be smeared with some fatty body, such as almond oil, so as to render its movements more soft and easy. The shampooing must be performed in the gentlest manner, without shocks, directed from below upwards, and acting not only on the painful points, but upon all those that are tumefied.

If pain is excited by an attempt at moving the joint, we must return to shampooing until new trials have proved that flexion and extension cease to excite painful sensations. Such movements would be very painful, or even dangerous, if performed at an early stage of the treatment. They do not, however, constitute any part of the treatment

itself, and are only resorted to as a means of appreciating the results derived from the shampooing. In several cases in which the cure had been considered as complete, the pains have returned next day, accompanied by more or less febrile reaction. A single re-application of the shampooing has sufficed to dissipate them, and in most cases, twenty-four hours' rest and the application of a bandage moistened with spirit of camphor, has sufficed for this. This bandage, indeed, is useful in all cases, and should be worn for two or three days.

This procedure is applicable to both old and recent sprains, and even when there has been present also a fracture of the fibula, shampooing has effected a remarkable diminution of pain and swelling, enabling the surgeon sooner to ascertain the exact nature of the case. Several cases of severe, recent, and old sprains are referred to, in which two or three hours of this shampooing process has effected an entire cure.

(E) CONCERNING OPERATIONS.

ART. 76.—*On Amputations.* By Mr. JOHN RUSSELL, of Merthyr Tydvil.

(*Glasgow Med. Journal*, Jan., 1859.)

In this paper Mr. Russell wishes to draw attention to the great difference between amputations performed on limbs which have been shrunk from disease and inaction, and those which are performed on sound and healthy limbs.

"A young operator, whose only experience has been on the dead subject, should he be called upon to operate upon a limb in the first condition above described, would find an absence of all elasticity of skin, cellular tissue, and muscle; and he would find no unequal retraction of parts, and probably no natural separation or retraction of them; and, should he have followed strictly the directions laid down by authorities, he would find the result correspond with his previous practice on the dead subject, and his operation would necessarily be followed by a flattering stump.

"But, in the case of a limb in the opposite condition, he would find circumstances very different, and be very likely disappointed at the result, as, in such a case, all structures would be found in the highest degree elastic, and the retraction of them very unequal. The skin would retract most, and the muscles (each layer) less and less towards the centre of the limb, thereby causing somewhat of a convex instead of a concave flap.

"The contrast between these two conditions has appeared to me so striking, that it surprises me so little notice has been taken of it. I have looked through Liston's, Skey's, and Erichsen's works on operative surgery, for suggestions applying to the different cases; but the only one bearing upon the point is the plan proposed by Mr. Skey in amputations (I believe?) in general; viz., that of pressing up the muscles under the protection of the bone with the flat hand before transfixing, preparatory to making the posterior flap. I have not tried this plan. It appears to

me, however, that, in so doing, the surgeon must sacrifice skin as well as muscle, although not in the ordinary proportions. And I fear that the palm of the hand, and more especially the ball of the thumb, must be inconveniently near the knife.

“My own plan has been to make two lateral incisions through the skin, then to grasp a fold of it between the finger and thumb (similar to the fold seized when lifting a dog by the nape of the neck), before transfixing for the posterior flap; and to carry the hand down, keeping pace with the knife, until sufficient flap has been secured; then to cut out. By this plan there is great gain of integument, and sacrifice of useless muscle; and the operator's hand is safely out of the way of the knife. The sacrifice of muscle will be found of great advantage; and the limb should be transfixed close to the margins of the drawn skin. It is hardly possible to save too much skin, for, however baglike and unseemly it may look at the time, it will eventually contract to the dimensions of the stump. The occasional difficulty of securing the vessels, after amputation of the leg, suggests the plan of carefully passing the knife perpendicularly and square to the plane of the bones, a short distance below the intended site of sawing through them, and then cutting up a short way close to each; thus leaving a tongue of muscle and ligament between the bones which would contain the vessels.”

ART. 77.—*On the causes of death after Amputation.*
By Mr. BRYANT, Assistant-Surgeon to Guy's Hospital.

(*Proc. of Roy. Med. and Chir. Soc.*, Feb. 22, 1859.)

This paper is based upon an analysis of 300 cases of amputation, collected from the records of Guy's Hospital. The author has divided them into four classes; and although he has not thought it necessary to alter the ordinary division of traumatic amputations into primary and secondary, he has made some change in the division of the other forms; for it became evident, in the analysis, that the classing together of such cases as amputations for talipes, tumours, elephantiasis, deformity, and others of a like character, with those of diseases of the joints, a wrong result must ensue; and, practically, this was found to be the case. He therefore divides these cases into pathological amputations and amputations of expediency, choosing the latter term as more accurately expressing the reason for the operation, as limbs are removed for tumour, talipes, elephantiasis, and deformity, more from expediency than necessity; and he therefore suggests the use of such a term until a better was proposed. The author then proceeds to an analysis of the table of amputations, including 167 cases of pathological amputations, 33 amputations of expediency, 76 primary amputations, 24 secondary amputations; and having given in detail the analysis of each division, he sums up the whole in the following general conclusions: 1. That in amputations of the extremities, taken altogether, 25 per cent. are fatal; 30 per cent. in the lower extremity, and 10 per cent. in the upper. 2. That amputations, as a whole, are fatal in the following order:—Secondary, 50 per cent.; primary, 43 per cent.; amputations of expediency, 30 per cent.; pathological amputations, 12·5 per cent. 3. That

in pathological amputations of the thigh 18 per cent. are fatal, or 1 in 5·5; leg, 7·7 per cent. are fatal, or 1 in 13·0; foot and upper extremity, success generally follows. 4. That in amputations of expediency of the thigh, 31·5 per cent. are fatal, or 1 case in 3·16; leg, 66·6 per cent. are fatal, or 1 case in 1·5; upper extremity, fatal cases are exceptional. 5. That in traumatic amputations of the lower extremity, 60 per cent. are fatal; of the upper, 18 per cent.; and that traumatic amputations of the leg are at least as fatal as those of the thigh. 6. That secondary amputations are more fatal than primary. 7. That in amputations of the thigh for chronic disease of the knee-joint, 1 case only out of 7 proves fatal,* or about 14·5 per cent.; but for acute suppuration, a fatal termination is the rule. 8. That in amputations of the lower extremity for tumours, 36 per cent. are fatal; of the upper, recovery may generally be expected. The author then proceeds to the more immediate subject of the causes of death after amputations; and having given two tables, showing the different causes of death, and their proportions or per centages both to the fatal cases and the whole number of amputations, he goes on to the analysis of each division, and having given a detailed account of each, he condenses the whole into the following conclusions:

General conclusions upon the causes of death in amputations generally.—

1. That 25 per cent. are fatal; 30 per cent. of the lower extremity, 10 per cent. of the upper.
2. That pyæmia is the cause of death in 42 per cent. of the fatal cases, and in 10 per cent. of the whole number amputated.
3. That exhaustion is the cause of death in 33 per cent. of the fatal cases, and in 8 per cent. of the whole number amputated.
4. That the following causes of death are fatal in the annexed proportions:

	Of fatal cases.	Of whole number.
Secondary hæmorrhage	7· per cent.	or 1·66 per cent.
Thoracic complications	5·6 "	1·33 "
Cerebral "	3· "	·66 "
Abdominal "	1·4 "	·33 "
Renal "	3· "	·66 "
Hectic "	3· "	·66 "
Traumatic "	7· "	1·66 "

Pathological amputations.—1. That pathological are by far the most successful amputations, 12·5 per cent. proving fatal. Such amputations of the upper extremity are generally followed by success. Of the lower extremity, 15 per cent. terminate fatally. 2. That pyæmia is the chief cause of death, proving fatal in 43 per cent. of the fatal cases, and in 5·4 per cent. of all pathological amputations; and when fatal, as a rule, it causes death within fourteen days of the operation. 3. That exhaustion, either from the shock of the accident or of the operation, from hæmorrhage, or all three causes combined, is the cause in 33 per cent. of the fatal cases, or 4 per cent. of all amputations. 4. That secondary hæmorrhage is the fatal cause in only 9 per cent. of the fatal cases, and in 1·4 per cent. of all amputations. 5. That hectic, abdo-

* The fatality of excision of the knee, according to Butcher, is at least 1 case in 5.

minal, and thoracic complications act equally as causes of death in 13 per cent. of the fatal cases, and in 2 per cent. of all amputations.

Amputations of expediency.—1. That 30 per cent. are fatal; but as amputations of the upper extremity are, as a rule, successful, the percentage of this operation upon the lower is much increased, 40 per cent. proving fatal. 2. That pyæmia is the chief cause of death, proving fatal in 60 per cent. of the fatal cases, and in 18 per cent. of all such amputations; and when fatal, as a rule, death takes place within fourteen days of the operation. 3. That death from exhaustion occurs in but 10 per cent. of the fatal cases; and that some thoracic or renal complication, or carcinomatous infiltration, are fatal causes in the same proportion.

Primary amputations.—1. That 43 per cent. are fatal; 60 per cent. of the lower extremity, and 30 per cent. of the upper. 2. That primary amputations are more successful than secondary. 3. That pyæmia is the cause of death in 43 per cent. of the fatal cases, and in 16 per cent. of the whole number, and, that, when fatal, the symptoms appear, as a rule, between the seventh and fourteenth days after the operation, and cause death in the third or fourth week, and not during the first two weeks, as in pathological amputations and those of expediency. 4. That exhaustion is the cause of death in 32 per cent. of the fatal cases, and in 12 per cent. of the whole number. 5. That traumatic complications prove fatal in 15 per cent. of the fatal cases, and secondary hæmorrhage, cerebral or thoracic complications, about 7 per cent. each; renal disease proving a cause of death in 3·5 per cent.

Secondary amputations.—1. That 50 per cent. are fatal; 68 per cent. of the lower extremity, and 12·5 per cent. of the upper. 2. That secondary amputations are more fatal than primary, by about 8 per cent. 3. That exhaustion is the chief cause of death, proving the cause in 60 per cent. of the fatal cases. 4. That pyæmia is the cause in 25 per cent. of the fatal cases; secondary hæmorrhage and hectic in the remaining 15 per cent.

Conclusions upon pyæmia as a cause of death.—1. That it is the cause of death in 42 per cent. of all fatal cases of amputations, and in 10 per cent. of all amputations. 2. That it is the cause of death in the different forms of amputation in the following order:—1. In 70 per cent. of all fatal amputations of expediency. 2. In 43 per cent. of all fatal primary amputations. 3. In 43 per cent. of all fatal pathological amputations. 4. In 25 per cent. of all fatal secondary amputations, and that in amputations of expediency it is the most frequent cause, and in secondary amputations the least. 3. That in amputations for acute suppuration of the knee-joint, whether the result of an abscess discharging into the joint or otherwise, pyæmia is a more frequent cause of death than in amputations for chronic disease. 4. That it is the general cause of death in amputations for talipes, elephantiasis, and tumours. 5. That in primary amputations, and in amputations of expediency of the leg, it is a more frequent cause of death than in the same operations upon the thigh. 6. That, upon the whole, pyæmia appears to be a more frequent cause of death in amputations through limbs the tissues of which are in a normal condition, and where a large surface of healthy bone is exposed. 7. That in pathological amputations, and in amputa-

tions of expediency, pyæmia, as a rule, proves fatal within fourteen days ; but, after traumatic amputations, the period of death is about the twenty-fifth or twenty-sixth day.

General conclusions upon amputations of the thigh.—1. That 27 per cent. are fatal. Pathological amputations, 18 per cent. ; amputations of expediency, 31 per cent. ; primary amputations, 60 per cent. ; secondary, 75 per cent. 2. That in amputations of the thigh for chronic disease of the knee joint, about 15 per cent. are fatal, or 1 case in 7. 3. That amputations of the thigh for acute suppuration in the joint are generally fatal ; and that pyæmia is the chief cause of death in these cases. 4. That exhaustion and pyæmia are causes of death in equal proportions, or in about 40 per cent. of the fatal cases ; and in 10 per cent. of all amputations of the thigh. 5. That exhaustion is most fatal in primary amputations, and the least so in amputations of expediency. 6. That pyæmia is most fatal in amputations of expediency, and the least so in primary. 7. That primary amputations are, for the most part, fatal from exhaustion, 35 per cent. of the cases sinking from this cause, 15 per cent. from pyæmia, and secondary hæmorrhage and traumatic complications 5 per cent. each. 8. That exhaustion, pyæmia, and hectic are equally fatal causes in secondary amputations, proving fatal in 25 per cent. each.

Amputations of the leg.—1. That 37 per cent. are fatal ; pathological amputations, 7·7 per cent. ; amputations of expediency, 66·6 per cent. ; primary amputations, 62·5 per cent. ; secondary amputations, 66·6 per cent. 2. That amputations of the leg are 10 per cent. more fatal than of the thigh ; the amputations of expediency and traumatic amputations being more fatal, and the latter more frequent. 3. That amputations of expediency of the leg are generally fatal, being twice as fatal as those of the thigh ; that pyæmia is the chief cause of death in 75 per cent. of the fatal cases, and in 50 per cent. of all such amputations. 4. That in primary amputations, pyæmia is the cause of death in half the fatal cases, or in 32 per cent. of all such operations ; exhaustion and visceral complications about 8 per cent. each. 5. That comparing primary amputations of the thigh and leg together, they are equally fatal ; but that pyæmia is twice as fatal in amputations of the leg as in amputations of the thigh. 6. That half the cases of secondary amputations die from exhaustion ; pyæmia and secondary hæmorrhage being fatal in 8 per cent. each. 7. That taking all amputations of the leg together, 42 per cent. of the fatal cases die from pyæmia, and 32 per cent. from exhaustion.

Amputations of the upper extremity.—1. That 10 per cent. are fatal. 2. That pathological amputations and those of expediency are, as a rule, successful. 3. That about 20 per cent. of traumatic amputations are fatal ; 22 per cent. of the arm, and 16 per cent. of the forearm. 4. That one third of these fatal cases die from pyæmia ; one third from some traumatic complication ; and the remaining third from secondary hæmorrhage or visceral disease.

(F) CONCERNING INSTRUMENTS, ETC.

ART. 78.—*On Iron-wire Sutures.* By Professor SIMPSON.*(Medical Times and Gazette, Jan. 1, 1859.)*

"Last summer," says Dr. Simpson, in a recent clinical lecture on vesico-vaginal fistula, "I took occasion to make an extensive series of experiments upon the relative merits of metallic inorganic sutures and ligatures, and upon the relative surgical qualities of different metallic threads. These experiments were for the most part kindly performed for me by my friends Mr. Edwards, Mr. Jardine Murray, and Dr. Coghill, and the subjects of the experiments were a number of unfortunate pigs, which were always, of course, first indulged with a good dose of chloroform. We made corresponding wounds of various kinds, usually on directly opposite sides of the body, and sewed some with threads of silk, hemp, cotton, &c., and others with threads of silver, gold, platinum, lead, iron, &c. As a general result the contrast between wounds sewed with organic threads, and wounds of corresponding size and situation upon the same animal, sewed with metallic threads, was most striking and remarkable. For while the silk and other organic sutures almost invariably began to inflame and suppurate along their tract a few days after their introduction, the metallic sutures remained, as it were, quite passive in the lips of the wounds, and without exciting any appreciable inflammatory disturbance. I have seen enough of cases in the human subject to convince me that the same comparative results as a general law follow the uses of these two forms of suture in the surgery of the human body. In fact, the surgeon is almost invariably obliged to cut out a silk or other organic thread a few days after its introduction, in consequence of the suppuration and ulceration which its detention excites. You may leave, on the other hand, a metallic suture without any such consequences for weeks or months, instead of days.

"Why do metallic threads not lead on to the higher degrees of inflammation, such as suppuration and ulceration, along their tracts and in their neighbourhood, as organic threads do? I believe this question is to be solved by the mere fact of metallic bodies or threads lying unchanged and inert in and among the tissues with which they are in contact. If we introduce a metallic wire into a part, it has no power of absorbing the fluids there, and lies in apposition to the tissues without irritating them. A thread of silk absorbs the fluids thrown out—lymph, or pus, or whatever else it might be—and these dead fluids remaining in the thread and becoming decomposed, render it a small tract or nidus of putrefaction and infection. In the experiments already alluded to, I repeatedly took silk threads which had been a few days in the lips of wounds in the pig, and had there produced suppuration in their tracts, and placed small portions of them in the bottom of new wounds in the same animal. Within a day or two, severe inflammation, sometimes of a carbuncular form, appeared in the lips and sides of these new wounds, showing the acrid and morbid nature of the dead and decomposing materials absorbed by and retained within these organic threads.

“What metal is best? This question has been often asked, and variously answered. Sims uses always silver wire in preference to any other. Mettauer, like Dieffenbach, operated with leaden wires. I have always used the ordinary simple and cheap blue iron-wire of the shops; and I believe it is the best. What is required is a material not readily oxidizable, and possessed of a certain degree of strength and tenacity. Now, it has been found that a certain thickness of wire, if made of lead, will sustain a weight of 1 lb.; silver, 9 lb.; platinum, 13 lb.; iron, 26 lb.; from which it will be seen that iron-wire will not so readily give way as some of the others. But is it from any particular reason more irritating than these, or more likely to produce a high, and dangerous, or destructive degree of inflammation? The liability of iron to rust, or become oxidized, at once occurs to most minds as likely to impair its usefulness, and render it irritating to the tissues with which it is brought into contact. But we know that iron in some forms does not become oxidized in the body, and causes no disturbance whatever in the tissue. Needles, for instance, usually excite little or no inflammation; and I show you here a portion of a needle removed by Mr. Murray from a child's foot, in which it had lain three years and four months without becoming in any degree roughened on the surface by rust or oxidation. And Schönbein has shown that, by being submitted to certain processes, iron may be rendered what he calls ‘passive;’ and in this passive state it is not at all liable to become changed and oxidized. For while iron, in the ordinary condition, gives rise to a sort of effervescence on being introduced into strong nitric acid, of specific gravity 1·3, this phenomenon is not seen when the iron is in the ‘passive’ state. This condition may be induced in various ways,—by passing a piece of wire through the flame of a spirit-lamp, by introducing the wire into nitric acid at the same moment, and in contact with a piece which is already passive, or, as in the wire which I commonly use, by annealing, which is done, I am told, by putting the hot wire into an oil bath. The wire, then, which I always use, and which I believe to be the best, as it certainly is the cheapest, is the ordinary annealed iron-wire of the shops, and of the size known as No. 32. I have by me here some specimens of iron-wire coated with tin, silver, &c., as well as wires of platinum and other metals; but not one of them fulfils any indication better than the simple annealed blue iron-wire, which may be bought at any wire-workers for a shilling a pound. Through the kindness of my esteemed friend, Dr. Aveling, of Sheffield, the firm of Cockers, Brothers, of that town, have lately manufactured an iron wire for surgical purposes, drawn out of the finest procurable material, and in this respect superior to the common iron thread I have always used.”

ART. 79.—*A new Fissure-needle.*

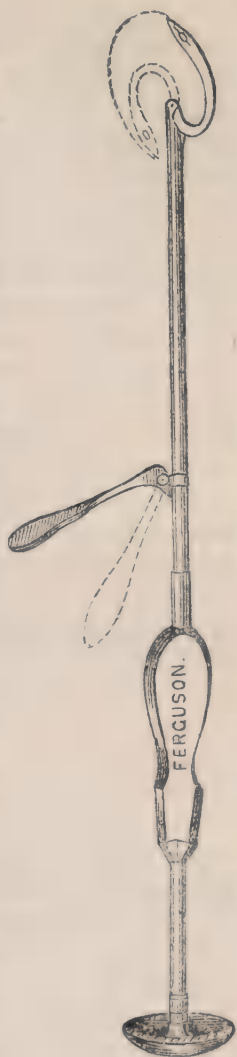
By Mr. FERGUSSON, Surgeon to King's College Hospital.

(*Medical Times and Gazette*, Nov. 20, 1858.)

The annexed woodcut will explain better than any verbal description how Mr. Fergusson has endeavoured to supply the want long felt by

operating surgeons of a convenient needle for passing a ligature easily and rapidly through the opposed surfaces of a fissure, more particularly in the operations for the cure of cleft palate and vaginal fistulæ.

The instrument consists of a handle and staff, both in the same straight line, a needle, curved ellipsoidally being fixed by an hinge-joint at the extremity of the staff. This needle is forced to move in the radius represented in our engraving by a dotted line, when pressure is made upon the projecting handle of a compound lever, which is attached at its furthest extremity to the needle aforesaid. When the edges of the fissure have been brought together in the usual manner, the surgeon is able to pass his silk or wire ligature through both sides of the opposed edges by a single movement; *i. e.* he thrusts the needle (carrying the silk) upwards so as to perforate one of the edges of the wound, and then carries it through the other side by simply pressing on the handle of the lever. Having seized the silk or wire with forceps, a removal of the pressure exerted by the thumb on the lever allows the needle to resume its former position, leaving the silk in the desired situation, and the instrument may then be withdrawn, re-threaded, and the same routine observed till the requisite number of stitches have been passed. At the suggestion of Mr. Spencer Wells, who used this needle in a case of vesico-vaginal fistula, Mr. Fergusson now makes the needle with an opening eye, so that the silk or wire may be freed as soon as passed, and the needle more easily withdrawn.



ART. 80.—*Are not the Escharotic preparations of Steel the best Escharotics in certain cases?* By Professor SIMPSON.

(*Medical Times and Gazette*, Feb. 5, 1859.)

“We have,” says Professor Simpson, “at least one very interesting case recorded to show that the simple muriate of iron may prove to be a most useful agent in the destruction of carcinomatous growths. I allude to a case occurring in the practice of my friend, Dr. Boulton, of Horncastle, who had a patient with epithelial cancer of the cervix uteri, which had attained a considerable size, which frequently was the source of profuse hæmorrhage, and which was quite friable, and broke down under examination with the finger. He applied some tincture of the muriate of iron to the broken-down tissues, and this seemed to act by coagulating the blood in the small blood-vessels of the part, and so destroying its nutrition that it mortified and sloughed away. Perhaps a saturated solution of the perchloride would act still more effectually.

The disease returned, and was again treated in the same way, and with the same result; and after it had in this way several times recurred and been destroyed, it was finally cured, and the patient has now remained well for several years. The observation is an extremely interesting one, for it may be that while arsenic would destroy the cancerous parts, and then pass deeper and exert its poisonous action on the whole body, iron or some other metal may have merely the more local effect of poisoning the morbidly disposed cells, in and near the diseased part. And in reference to this matter you must remember that iron, in its various forms, is also one of the best of all tonics that you can prescribe for a cancer-affected patient. There is evidently in such cases a diminution of the quantity of iron in the blood, as is shown also by chemical analysis; they are usually chlorotic; and so convinced was Mr. Carmichael, of Dublin, of the beneficial effects of iron in the treatment of cancer that he proposed to keep patients saturated with it as the best means of checking the progress of the disease. It certainly is one of the best tonics we possess for such cases; and a few instances like that narrated by Dr. Boulton would show it also to be a very admirable escharotic, and might lead to its more general application for the local destruction of the carcinomatous growth."

ART. 81.—*On Sulphate of Zinc and Sulphuric Acid as an Escharotic.*
By Professor SIMPSON.

(*Medical Times and Gazette*, Feb. 5, 1859.)

In a clinical lecture on carcinoma of the uterus and mamma and its treatment by caustics, we find the following very important remarks:

"If you make use of the caustic made by saturating strong sulphuric acid with a quantity of sulphate of zinc, dried and powdered, you can manage by dipping an ordinary quill pen in this mixture, as if you were going to write with it, to lay it in a number of lines across the tumour, the number of lines corresponding to the size of the growth which you wish to destroy. Very speedily the super-sulphate of zinc kills the skin in the course of the lines which you have drawn; and if you will now scratch assiduously with the filled pen along these lines, you will cut through the skin in a few seconds. Leave for a day the fissures filled with the caustic paste, and then every day or two by renewing the application of the scratching and caustic you can cut down to a greater depth. In making the first application I usually make a fissure of about a fourth or three eighths of an inch in depth, and then destroy the tumour more deeply by successive applications. In this way five or six days may suffice for the removal of a good-sized tumour. Let me add, that in thus destroying and digging out, as it were, from its very base, a cancerous tumour of the mamma, or other external part—neither of the two caustics applied by the quill would suffice singly and individually. If you used the sulphuric acid alone you will find that the acid so chars and hardens the spot to which it is applied that you cannot next day, cut or scratch deeper through it with the pen; while the super-sulphate of zinc paste keeps the parts soft and pliable. Again, if you used sulphate of zinc alone you could not cut through the skin or penetrate

deeply with it. For that purpose the aid of the sulphuric acid is required, and the relatively slighter pain attendant upon this than upon most other caustics is perhaps explicable by the fact that sulphuric acid acts almost instantaneously in producing its destructive effects upon living tissues. Usually the healthy skin at the edge of the sloughing-out mass is granulating, contracting, and partially cicatrising before the dead tumour itself separates. Dress the exposed tissues or ulcer before and after the tumour is enucleated with black wash, chloride of zinc, sulphate of zinc, nitrate of silver, or any other appropriate surgical lotion."

II.—SPECIAL QUESTIONS IN SURGERY.

(A) CONCERNING THE HEAD AND NECK.

ART. 82.—*Cases of Granular Ophthalmia treated by the application of gonorrhœal matter to the eye.* By Mr. HUMPHRY, Surgeon to Addenbrooke's Hospital, Cambridge.

(*British Med. Journal*, Jan. 8, 1859.)

Granular ophthalmia is without doubt one of the *opprobria* of surgery, and daily experience proves sufficiently that it is little influenced by ordinary means of treatment. Mr. Humphry was prepared therefore to give a favorable consideration even to the suggestion of the somewhat revolting and hazardous experiment of applying gonorrhœal matter to the eye, which had been already carried out by Mr. Bowman at the Moorfields Eye Infirmary. The principle obviously is that of curing a chronic inflammation and its results by exciting a more severe inflammation in the structure; and, as no other agent that he could think of was likely to produce an impression sufficiently severe and prolonged to work the desired effect, Mr. Humphry determined to introduce the gonorrhœal matter into one eye of each patient. It is worthy of remark, that acute inflammation of both eyes quickly followed. The result has been most satisfactory, inasmuch as good sight has been restored in one, and very fair sight in the other of two persons, who were nearly blind, and apparently condemned to remain so. It need scarcely be observed, that such a mode of treatment should be reserved for the very worst cases, those in which the cornea has become so opaque that useful vision is lost, and in which a fair trial has been given to milder means.

CASE 1.—Wm. Ford, æt. 49, a thin pale man, had been the subject of ophthalmia for more than three years. The disease was, he believes, caused by particles of sand getting into his eyes when following the occupation of digging for coprolites; it was at first neglected, then treated with stimulating applications, and gradually assumed a very aggravated form. The conjunctiva of the lids and of the hinder part of the globe in each eye was in a highly granulated and vascular state; the surface of the right cornea was so opaque

that he could scarcely distinguish light from darkness; and in the left cornea there was not sufficient transparency to enable him to guide himself; the lids extremely relaxed. He had undergone a great variety of treatment, in and out of hospital, both in Cambridge and in London, without deriving the least benefit; and there was much reason to fear that he would be blind for the remainder of his life.

July 17th, 1858.—Some fresh gonorrhœal matter was taken from the penis of a man, and applied to the conjunctiva of the *right* eye. This was followed by very severe purulent ophthalmia of *both* eyes, the conjunctivæ swelling, and the surfaces of both corneæ becoming quite vascular. Fomentations and frequent washings with warm water were used. Gradually the inflammation diminished; the conjunctival swelling subsided; the purulent discharge ceased; and the corneal structure again came into view, and became more and more clear. The left cornea is now nearly transparent, and the right is more so than it has been since the first commencement of his malady. He can read large print, and is able to commence working upon the roads. The granulations upon the conjunctivæ are very much less large and numerous; and it is hoped the improvement will continue.

CASE 2.—Catherine Coyfield, æt. 14, a healthy girl, had suffered under ophthalmia for four years. The disease, at first neglected, and then treated with nitrate of silver, had become intractable, and resisted the ordinary remedies, employed with great care by the different surgeons of the hospital under whom she had been, at intervals, as in- and out-patient, for more than two years. The conjunctiva of the lids and back of the globe was very granular, and the corneæ were so opaque and vascular that she could scarcely see to guide herself; so that she was quite unfit for any work. Her condition was a source of great anxiety, the result of past treatment leading to the apprehension that no remedy would be found.

July 17th, 1858.—Some pus was taken from the eye of the gonorrhœal patient above mentioned, which had recently been destroyed by ophthalmia, and applied to the conjunctiva of the girl's *right* eye. Purulent ophthalmia ensued in *both* eyes, very severe, but not quite so severe as in the case of Ford. It gradually subsided; and the corneæ became clearer, so that by the end of August she could guide herself about very well. The improvement has continued; and she has for some time been able to read small print, thread her needle, do needle and other household work, and, indeed, see very well. The corneæ have nearly their natural transparency; and the conjunctiva has an almost healthy condition.

ART. 83.—*On the treatment of obstinate Granular Ophthalmia by the local application of a solution of Chromic Acid.* By Dr. HAIRION.

(*Archiv. Belge de Med. Militaire*, Sept., 1858; and *Gaz. Hebdom. de Méd. et Chir.*, Jan. 7, 1859.)

The solution employed by Dr. Hairion consists of equal parts of chromic acid and distilled water. This is applied by means of a camel-hair pencil, and the application, he tells us, is neither very painful, nor followed by any great amount of reaction. Fourteen cases were treated in this manner, and in eleven of these the result was satisfactory. The cauterizations were made at intervals of four, six, or eight days; they varied in number from two to fourteen; and the time occupied in the cure extended from four weeks to four months. As might be expected,

considerable prudence is required in the adoption of this mode of treatment, and it is only in *very obstinate* cases that it is recommended.

ART. 84.—*On a new method for the rapid destruction of Conjunctival Granulations.* By Dr. BORELLI.

(*Presse Méd. Belge*; and *Dublin Medical Press*, March 30, 1859.)

After some preliminary remarks, M. Borelli goes on to the description of a new instrument proposed by him for the destruction of the granular tissue. It consists of a brush of iron with very sharp points, straight and long, like the teeth of the comb used for carding wool. These points, applied with force to the granulations, penetrate them in their whole depth, and detach them from their base as the instrument runs over them.

We leave the author to explain his method of operation.

The patient being seated in a chair facing the light, or, if lying, with his head well supported by pillows, the upper lid is everted in the ordinary manner, taking care to turn out the two angles as much as possible, as they are the principal seat of the granulations. The lower lid is then drawn down by an assistant, so as to evert the conjunctiva. Then the operator, with his right hand, applies the points of the instrument to the granulations, employing the greatest number possible, and passing them three or four times over the affected parts with the requisite pressure. The pain is not excessive, and ceases almost immediately, in which it differs from that induced by cauterization. The abstraction of a considerable quantity of blood which follows on these deep incisions produces immediate relief by getting rid of the congestion which always accompanies granulations. When the operation is over, and the lids turned down, cold-water dressing is applied to the eyes, and the whole is finished.

This is the operation which is requisite in cases where the granulations are numerous and occupy nearly the whole mucous surface of both lids. But where the disease is confined to one lid, or only to the angles, the operation must be limited to these regions, and must be effected with the angular extremity of the instrument which acts with the same certainty.

All the palpebral granulations cannot, however, be removed by this operation; a few will often remain hidden deeply on the fold of the upper lid, and which cannot be discovered by the eversion of the lid, and so escape from the point of the instrument. To destroy these it will be necessary to use a "brush," the free extremity of which is straight and quadrangular; it must first be passed between the globe of the eye and the lid, against the fold of mucous membrane, towards which the points of course must be directed, and then drawn down rapidly from one angle to the other.

The destruction of the granulations may be repeated every second, third, or fourth day, according to the results obtained. M. Borelli has established the fact that, subsequent to the destruction of the granulations from the base by the application of the instrument, cauterization with sulphate of copper may prove useful for the complete eradication of

the granular germ, and to prevent the reproduction of their primary anatomical elements.

The organic changes he has observed on the mucous surface of the lids subdued by an amount of laceration sufficient for the final cure of the granulations are, first, the deposition of grayish or yellowish coagulable lymph, which constitutes the first traces of a plastic reconstruction, then the formation of that lymph into a more compact, homogeneous, and shining tissue, which becomes vascularized, and acquires little by little the nature of mixed cicatrix. This favorable result is due to the fact, that the fundamental mucous tissue has not been entirely destroyed by the operation for the removal of the granulations.

We may remark that in the paper about to be published in the next number of the 'Journal d'Ophthalmologie de Turin,' our learned brother Borelli, in order to show the importance and utility of his treatment, has founded his arguments on the principles set forth by Professor Thiry as to the nature, causes, and alterations which characterise true granulations, which are specific, and only give place to a contagious and virulent product. The means extolled by M. Borelli only apply to true granulations, and not to those common elevations produced oftener by a simple, congestive, inflammatory development of certain normal elements of the conjunctiva, such as the papillæ and follicles, and which some authors have so arbitrarily transformed into the fleshy vesicular granulations proper to Belgian military ophthalmia. These pretended granulations, as all the world now knows, only require a sufficiently energetic treatment; they disappear of themselves, or are cured by the simplest agents, whilst formerly they were looked on as dreadful bug-bears, requiring to be at once extirpated.

ART. 85.—*Sulphur-ointment as a remedy in Granular-Conjunctiva.* By Mr. WHARTON JONES, Ophthalmic Surgeon to University College Hospital, London.

(*Medical Times and Gazette*, Jan. 15, 1859.)

Mr. Wharton Jones tells us that he has used sulphur ointment as a remedy in granular conjunctiva with very encouraging results. After scarifying the inflamed surface, he applies the ointment in the following manner:

A piece the size of a split pea is to be taken up on the point of a probe, or on the point of the nail of the little finger of the right hand, and insinuated under the upper eyelid, while this is drawn forward from contact with the eyeball. When the salve is fairly on the eye, the upper eyelid is to be gently drawn down and rubbed over the eyeball with the finger for a minute or so, in order to diffuse the salve, now melted by the heat of the eye, between the eyelids and eyeball, and consequently all over the conjunctiva.

The sulphur ointment causes rather more pain than the red precipitate ointment.

"Marked as the good effects of the sulphur ointment have been found to be so far, it remains," he says, "for experience to show whether we shall be indebted to it for anything like a complete and

permanent cure of that miserable complaint of the eyes—granular conjunctiva.

“Reflecting on the peculiarly inveterate character of granular conjunctiva, the idea occurred to me that the disease might possibly depend on, or at least be kept up by the existence of some parasitical organism, and that accordingly sulphur might prove a remedy. I was thus led to try the Unguentum Sulphuris (the most convenient form of the remedy for the purpose), instead of the red precipitate ointment which I had previously been in the habit of using.

“Whether this conjecture shall turn out well founded or not, is a matter of comparatively small consequence, so long as the remedy which it suggested is useful.”

ART. 86.—*Formation of an artificial Pupil by tying the Iris.*
By Mr. CRITCHETT.

(*Ophthalmic Hospital Reports, Oct., 1858.*)

The object of this operation (which Mr. Critchett terms *iridodesis*, from *ἰρις*, and *δεσις*, a binding) is to prevent the receding of the portion of the iris drawn through the cornea in the operation for artificial pupil. This operation is performed in the following manner. The patient, if at all restless, being placed under the influence of chloroform, the wire speculum is inserted, and, with a pair of forceps, a small fold of the conjunctiva close to the cornea, is held so as to fix the eye. An opening is then made with a broad needle through the margin of the cornea, *close* to the sclerotic, and just of sufficient size to admit the canula forceps; with it a small portion of the iris, near, but not close to, its ciliary attachment, is seized and drawn out to the extent considered necessary to enlarge the pupil; a piece of fine floss silk, previously tied in a small loop round the canula forceps, is slipped down and carefully tightened around the portion of iris made to prolapse, so as to include and strangulate it. This manœuvre requires a little practice and dexterity, and is best accomplished by holding each end of the silk with a pair of small forceps with broad extremities, bringing them exactly to the place where the knot is to be tied, and then drawing it moderately tight. A single tie is sufficient; the ends are then cut off, and the operation is complete. Little or no irritation usually follows. The small portion of iris included in the ligature speedily shrinks, leaving the little loop of silk, which may be removed from the eye about the second day. This operation has been performed many times by Mr. Critchett, and by his colleagues, Mr. Bowman and Mr. Poland, and the result has been in every respect most satisfactory; the size, form, and direction of the pupil, can be regulated to a nicety; its mobility is preserved, and the eye speedily recovers from the effects of the operation. It is applicable to numerous groups of cases, including all those in which the natural pupil, or even a part of the natural pupil, is moveable, and has a free edge.

ART. 87.—*On Ophthalmostasis, with an account of an improved method of extracting cataract.* By Mr. JOHN F. FRANCE, Ophthalmic Surgeon to Guy's Hospital.

(*Guy's Hospital Reports*, 1858.)

"For many years," says Mr. France, "I have been accustomed to steady the eye during extraction by the contact and pressure of the fingers alone, according to the practice of most modern operators; the fore finger holding the upper lid, and restraining the globe's movement upwards, the middle finger on the caruncle curbing its movement inwards. In many cases this arrangement is sufficient for the purpose, and the section is made not only satisfactorily but with ease. In how great a degree, however, that ease is dependent on the patient's strength of nerve and steadiness of eye; and how limited the surgeon's real command of the globe is apt to prove, when the opposite qualities are manifested (especially if the anatomical conformation of the parts happens at the same time to be unfavorable), every operator of wide experience and equal candour must confess. Can no unobjectionable means, then, be devised which shall render his command absolute?"

"In operating for the formation of artificial pupil, I first became aware of the practicability of holding the eye perfectly still and motionless, or as nearly so as possible, by the mere application of artery forceps. The idea at length was suggested of extending the use of this instrument to another operation, in which, as far as I know, it had never been employed (at least in this country) before; of availing myself, in short, of the same resource as in cases of artificial pupil (and with a similar object) in cases of extraction.

"I have since brought the idea to the test of experience, with the result which it is my present object to make known,—the result, that is, of facilitating in a degree I could not have anticipated, the most critical stage of this operation. The mode in which I proceed is as follows. As soon as the patient is laid on the operating table, and all the preparations are complete, standing at his head, I apply the extremity of the forceps with rather firm pressure a little beneath the inferior margin of the cornea, and clasp a somewhat broad portion of conjunctiva and of the submucous fascia securely. Then, taking the instrument between the finger and thumb of the other hand, as near as practicable to its closed points, I deliver it to the assistant; whose hand, supported upon the patient's cheek, receives it, and holds it as he would a pen. It is well that the assistant should be practised in his share of duty on the dead subject. The ordinary artery forceps are, on the whole, preferable to those with a spring catch, commonly known as Liston's; but it is of consequence that the nibs should be broad, and the teeth sufficiently prominent. The lower lid requires no further depression than that necessarily produced by the attachment of the instrument to the ball in this way. I then raise the upper lid with the fore finger, direct the assistant to draw the cornea into a central position and retain it there with the forceps, place my fore and middle fingers on the globe in the usual way, and thus perfect the command of the organ. On now making the section, the eye is found steady and motionless; the knife can be deliberately entered, deliberately carried across the chamber, and de-

liberately brought out on the inner side of the cornea; and counter punctation being fully effected, and the flap on the verge of completion, the object of the forceps is accomplished, and they are at once disengaged. The remainder of the operation is finished in the ordinary manner.

"I have had much experience in the operation of extraction, having performed it myself considerably upwards of a hundred times; and of course am familiar with its pleasures (so to speak), and its difficulties, its contingent casualties, and the sequelæ of embarrassment attending them. After one or two trials, therefore, I was in a position to estimate the amount of advantage gained by the accessory manipulation just described; and this has proved indisputably so great, that I have employed the forceps without exception ever since. It will, perhaps, be the best way to place before the reader the facts which the current season has enabled me to gather, as practical criteria of the soundness of my conclusions."

Mr. France then gives twenty cases in which he has operated during the present season. In all of these the forceps appears to have been in an eminent degree advantageous.

"They facilitated the operation in difficult cases, in exact proportion to their previous difficulty and risk. This was strikingly evinced in Cases 11 and 16; both of which patients had, at a former period, occasioned the utmost anxiety, from the impediments their restless eyes presented to the satisfactory performance of the operation. They, indeed, recovered their sight well, but it was in spite of dangerous obstacles. This year they each recovered the sight of the second eye, under circumstances strongly contrasted; that which was so embarrassing before was now effectually surmounted at the very outset, by the simple means we are considering. Still more remarkable in some respects is Case 17, that of the sailor; who on two previous occasions had proved so utterly uncontrollable, that I was compelled to desist from the attempt to operate. On the present occasion, with the aid of forceps, after a little preliminary trouble, the globe was brought into and held in a sufficiently favorable position for a good section to be made. Could more convincing evidence be afforded of the value of this appliance?"

"The assistance rendered by the forceps is further illustrated by the fact, that in not a single case did premature escape of the aqueous humour (with its attendant inconvenience of the iris folding over the knife, and forbidding the immediate completion of the section on pain of wounding that membrane) occur. In three cases only did the first incision prove from any cause inadequate, and resort to the bistoury become expedient; and it was in one of these alone (in manipulating the bistoury after the forceps were detached) that the iris was sensibly touched.

"I am well convinced that the advantages of the mode of operating now recommended will be fully appreciated upon trial: it may, therefore, be better for me to bestow a word or two in anticipation of any objection which might perhaps deter some one from the experiment. Really, the single objection which occurs to me as sufficiently plausible to merit notice (if the directions above given be followed, and especially that of disengaging the forceps as soon as counter-puncturation is complete, and before cutting out), is, that the conjunctiva might possibly suffer injury from the forceps sufficient to awaken troublesome inflammation, and com-

promise the result. A conclusive answer is afforded by experience: no mischief has, in any case, under my observation, ensued; and this fact is substantiated, not by the foregoing cases alone, but also by numberless cases of artificial pupil, in which forceps are now habitually used, as well as by many others of soft cataract (to which I have latterly extended their use), and which are all equally available for the determination of this point.

"It is right that I should acknowledge myself indebted to the work of Desmarres, already referred to, for the *idea* of adapting artery forceps as an 'ophthalmostat' in extraction, but not for their *advocacy*."

ART. 88.—*Reclination of Cataract with two needles.*

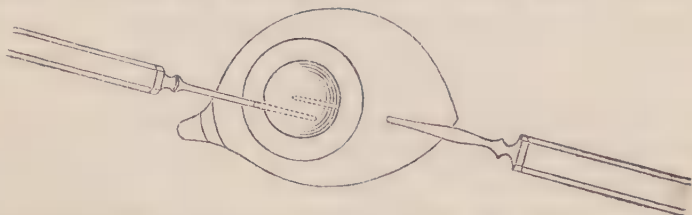
By Mr. J. V. SOLOMON.

(*Ophthalmic Hospital Reports*, Oct., 1858.)

In this article Mr. Solomon describes a method of employing two needles in "reclination" of cataract. He observes that the permanent results of this operation are most favorable where the eyeball is normally of large size, and the cataract consists of a hard nucleus enveloped in soft lens matter. This kind of cataract presents a broad edge and a deep antero-posterior diameter. Being in close contact with the ciliary body, and with the iris, to which its capsule is not unfrequently adherent, it obliterates the posterior, and sometimes encroaches on the anterior chamber. Hence the passage of a needle (entered through the sclerotic) over the rim of the lens to the centre of the pupil is difficult, inasmuch as the instrument is very apt to spit the lens or the ciliary body, or to be caught under the capsule of the lens or in the tissue of the iris.

Having dissected the eyes of a patient on whom reclination had been performed, Mr. Solomon was induced to examine whether the occurrence of the accidents above mentioned might not be obviated: and he was led to the conclusion that the desired object would be attained by pressing back the outer and lower side of the lens from the iris, with a fine needle passed through the nasal side of the cornea, thus making a free space for the sclerotic needle to pass to the front of the cataract.

Other advantages which appeared likely to arise from the use of two needles, were an increased facility in the removal of the soft portion of the cataract previously to the depression of the more solid portion; protection of the iris from contusion; and an effectual obstacle during the operation to the dislocation of the cataract into the anterior chamber. Mr. Solomon states that all his expectations were fulfilled when he operated with two needles; and moreover, that the lens was more easily



and methodically reclined, and was capable of being placed with greater exactness in the outer and lower fourth of the vitreous humour than in cases where only one needle was used.

ART. 89.—*Simultaneous development of Senile Cataract in Twins.*

By Mr. DIXON, Surgeon to the Royal Ophthalmic Hospital.

(Medical Times and Gazette, Nov. 13, 1858.)

An interesting example of extreme closeness in resemblance to each other in twins was presented by two men recently under Mr. Dixon's care, in the Ophthalmic Hospital. The twin brothers came up together from a village in Bedfordshire, to be treated for cataract. The age of each was 54, and both were in good health. Their similarity in physiognomy was so close, that when apart it would have been impossible to recognise the difference. Both were partially bald, and both had black hair, in which a few white ones were sprinkled. The degree of baldness, the amount of whisker, &c., in the one had their counterparts almost to a hair in the other. Both had lost a good many teeth, and although their mouths did not exactly coincide as to those which remained, they did so with one or two exceptions. The lower incisors were peculiarly crowded and irregularly placed in both. When seen together, one was observed to be about half an inch taller than the other, and this was the only point by which it was possible to distinguish them. Both had cataract, and in each it was advanced to the same stage, and was the more complete in the left eye. In one the disease had been advancing for two years and a half, and in the other for one year and a half. Mr. Dixon operated by extraction on the left eye of each brother on the same morning. The cataract of the one as closely resembled that of the other as could well be imagined, and in each the exterior of the lens was rather soft, and was squeezed off in the evulsion. The parallelism was maintained in the after progress, and when the men left the hospital, it would have been difficult to say which eye was the more perfect. In both the pupil was round and mobile, and the corneal section well healed; there was, however, in one a film of soft matter remaining, which failed its counterpart in the other. The right lens in both brothers is slightly opaque, and to about an equal degree. Both have followed the trade of butcher, both are married, and neither has had more than one child. They have sisters who are also twins, but the family resemblance is said not to be nearly so closely marked in the latter.

ART. 90.—*On Dacryops or Lachrymal Cysts.* By Mr. HULKE.

(Ophthalmic Hospital Reports, No. 6, Jan., 1859.)

Cysts in the upper eyelid, in connexion with the excretory ducts of the lachrymal gland, and occasionally complicated by fistulous openings upon the cutaneous surface of the lid towards its temporal angle, were first accurately described by Dr. J. A. Schmidt, in a treatise on the diseases of the lachrymal gland, published in Vienna, 1803, in which two cases, that had fallen under his own notice, are mentioned. Beer, in the second volume of his "*Lehre von den Augenkrankheiten*," 1817, says that he had seen six cases of this kind. He describes lachrymal cysts under the name of Dacryops, which Schmidt had applied to them; and he also gives a short account of true lachrymal fistula, but he appears

to have imperfectly understood its morbid anatomy. Since his time the affection has been mentioned in most of the larger systematic works on the diseases of the eye and its appendages; but very few authors seem to have written from a personal acquaintance with the subject, a fact which proves its extreme rarity.

Dacryops commences as a small cystic tumour, first noticed in the upper and outer part of the upper eyelid. The skin glides freely over it, but the tumour extends backwards beneath the border of the orbit towards the lachrymal gland. If the lid be drawn up on to the brow, and pressure be simultaneously applied in a downward and inward direction, a tense, elastic, fluctuating swelling instantly starts out between the eyeball and the inner surface of the eyelid. So long as the cyst is small the natural movements of the eyeball are unrestricted; but when it has attained a large size, and especially if it has reached backwards beneath the margin of the orbit, it hinders the movements of the globe, and may even cause its protrusion. The most characteristic and striking sign of *dacryops* is the sudden enlargement which the tumour undergoes when the patient cries. An attempt to extirpate such a tumour with the knife will generally fail, because the cyst is so delicate that it is hardly possible to dissect it away entirely, and if the wound should heal a tumour soon forms again. More frequently the wound does not perfectly heal, but a very minute fistulous aperture remains permanently open in the skin of the eyelid, from which the limpid secretion of the lachrymal gland continually oozes drop by drop. In this case the cyst causes little or no swelling in the lid, because the tears escape freely and do not distend it.

Lachrymal cysts are most commonly a consequence of mismanaged abscess, or of neglected wounds, attended by prolonged suppuration in the upper eyelid. The first step in their formation is the obstruction of one or more of the excretory ducts of the lachrymal gland, either at the external orifices in the upper sulcus of the conjunctiva, or at some other point nearer the gland. The escape of the tears being prevented they accumulate and distend that portion of the duct above the obstructed point, in connexion with the lachrymal gland. But these cysts have not invariably a traumatic origin; they are sometimes congenital. Schmidt took this view of two cases which came under his notice. He imagined that, by a congenital malformation, some of the excretory ducts terminated in the cellular tissue of the upper eyelid; that the secretion of the lachrymal gland was poured into the interstices and formed a kind of cyst by distending one or more such space which, as it enlarged, acquired a capsule by the condensation of the surrounding cellular tissue. Thus the formation of lachrymal cysts has been explained in two ways: one is by the uniform distension and dilatation of an excretory duct; the other by the distension and expansion of the interstices of the cellular tissue. But whatever their origin may be, it has no practical bearing on the treatment; and it is very important to bear in mind that, if these cysts be opened through the skin, a permanent fistula will probably result, and this may become the most prominent symptom, especially if the escape of the tears allows the cyst to collapse.

The successful treatment of *dacryops*, with or without fistula, depends on the artificial formation of a permanent opening for the escape of the

tears on the inner surface of the upper eyelid ; and when this has been accomplished the closure of the fistulous orifice on the skin can be attained without difficulty.

CASE 1.—Caroline Bewley, æt. 27, a glove-maker, applied to Mr. Bowman, at the Moorfields Hospital, May 25th, 1856, on account of a troublesome trickling of tears from a small hole in the skin of the upper lid of the left eye. Her mother had previously brought her to the hospital, when she was a child of nine years old, with a swelling in the upper eyelid, which was, at first, said to be a tumour ; but it afterwards suppurated, was lanced, and a hard body, resembling a plum-stone in form and size, was removed from it. The wound did not completely heal, and from that time tears have always oozed from a minute aperture in the skin of the lid ; they ceased to flow for a short time eight years before she came under Mr. Bowman's care. This cessation was followed by an abscess in the upper eyelid, and both the eyelids became so greatly swollen as completely to hide the eye itself. When the abscess broke, all the swelling of the lids subsided, but the fistula reappeared, and tears trickled from it as before. When she came to the hospital in 1856, I made the following memorandum of her condition at that time :

"In the skin of the upper eyelid of the left eye, near the outer end of the lid, and at about one eighth of an inch from its free border, there is a small orifice in the skin, from which a colourless limpid fluid continually oozes drop by drop and trickles down her cheek. It has a faint alkaline reaction, perfectly resembles tears, and does not excoriate the parts over which it flows. The aperture just allows the introduction of a fine probe, which can be passed for about half an inch upwards towards the lachrymal gland, and a careful examination shows that the fistula communicates with a cyst which occupies the outer half of the upper eyelid, but being collapsed does not cause any swelling. The cornea has its natural brightness ; there is not any redness of the conjunctiva, nor does she complain of any sensation of dryness in the eye."

The connexion of this cyst with the excretory ducts of the lachrymal gland could not be doubted, and it immediately became apparent that some provision must be made for the escape of the tears on the inner surface of the lid before the closure of the orifice in the skin could be undertaken with success. This was accomplished with a seton in the following manner :

A single thread of silk was armed with a needle at each end, and one of the needles was introduced into the fistulous orifice in the skin on the outer surface of the eyelid, and carried for a short distance upwards ; it was then made to pierce the fibro-cartilage of the lid and the conjunctiva, and the thread was drawn out at the inner surface of the lid. A similar manœuvre was repeated with the other needle, and the thread was drawn out at the inner surface of the lid at the distance of a quarter of an inch from the first, and a little nearer the attached border of the lid. In this way the cyst was pierced at two points by the thread which encircled in a loop the small intervening portion of tissue. The two ends of the thread were then brought out at the outer commissure and secured upon the temple with a piece of sticking plaster.

The presence of the thread occasioned very slight annoyance ; the conjunctiva, lining the upper eyelid, became a little swollen and injected ; and tears continued to flow from the orifice in the skin, but less abundantly. Ten days afterwards the thread was replaced by a thicker one, which produced more irritation, and the conjunctiva immediately around it became slightly granular. An attempt was now made to close the aperture in the skin. It was drawn out with a forcep and cut off with scissors, together

with the little piece of skin immediately around it. The fibres of the orbicularis palpebrarum were then seen covering the outer surface of the cyst, which was extremely thin. The edges of the wound were brought together with two *serres fines*, which were replaced in the evening of the same day by slips of plaster.

When she was next seen, after an interval of four days, the wound had quite healed, and the fistula in the cutaneous surface of the lid had perfectly closed. No tears had accumulated in the cyst, but one week after the operation a small quantity of mucus had collected in it; this could be easily squeezed out by the side of the thread through the opening on the inner surface of the lid. The thread was now withdrawn, and the small bridge of tissue which had been encircled by the loop cut out. This opening in the conjunctiva continued patent, and there was no further collection of mucus, nor of tears in the cyst.

Mr. Hulke also relates five other cases of dacryops — two from Schmidt, one from Beer, and two from Jarjavay.

ART. 91.—*On two new methods of treating diseases of the Lachrymal Sac.*
By Dr. v. GRAEFE.

(*Allg. Central Med. Zeitung*, No. 67; and *N. American Medico-Chir. Rev.*, March, 1859.)

In one of the sessions of the Society of Physicians of Berlin (July), Dr. v. Gräfe reported on two methods of treating affections of the lachrymal sac, which he considers a decided progress in ophthalmic surgery. One of them was proposed by Bowman, and has for its object the restoration of the permeability of the lachrymal passages by methodic dilatation. It differs from all the known methods of dilatation in the point that the lachrymal sac is not laid open through the skin, but that the instruments are introduced from the mucous membrane through the inferior punctum lachrymale, which has been previously dilated by slitting it. Although an experience of only four months does not permit any positive statement on the permanency of cures thus obtained, Dr. v. Gräfe does not hesitate, even at this early moment, to pronounce Bowman's method the best of all used for the restoration of the lachrymal passages.

The second innovation was proposed by Dr. Taignot, and has the opposite indication in view, viz., to destroy the lachrymal passages. Believing that the entrance of tears rendered the obliteration of the lachrymal sac difficult, Taignot recommends to cut off the puncta lachrymalia, in order to prevent tears passing into the sac. The idea itself is correct, but the obliteration of the lachrymal canals is not effected with certainty by the process recommended. Von Gräfe uses other means, for instance, ligation with a suture, gradually cutting through, or cauterization by means of small *portes caustiques*, which are introduced into the lachrymal canals. Dr. Leibreich, who assisted in Gräfe's clinic, conceived the idea of coating Anel's probes with nitrate of silver; in order to make the caustic adhere, the probes were first rendered rough by exposing them to the action of nitric acid; thus prepared, they were dipped into fused nitrate of silver. Any silver instrument can be converted by this process into a caustic body. After per-

meability of the lachrymal canals is obtained, the obliteration of the lachrymal sac is easily effected by gentle caustics. The hot iron, chloride of zinc, Vienna paste, &c., which often produce circumscribed caries, can be dispensed with.

By these two innovations, the old contest between destructive and conservative surgery, in the treatment of diseases of the lachrymal sac, has been revived. According to Dr. v. Gräfe's opinion, the following rules are to be observed in regard to the indications:—1. In every case in which circumstances offer the prospect that perviousness may be permanently restored, the surgeon should endeavour to obtain it by Bowman's method. 2. In cases where the restoration of permeability is problematic, and could only be obtained by a tedious cure, it must be ascertained whether the lachrymal glands of the patient, after removal of all causes stimulating them to excessive secretion, furnish a relatively great or small quantity of tears. Dr. v. Gräfe gives the necessary rules for making this estimate. In cases in which the quantity of the secretion is small, obliteration, after cauterizing the lachrymal canals, is preferable to restoration of the continuity. No *stillicidium lachrymarum* remains in this case. If, however, the quantity of the secretion is large, Bowman's method should be first tried, for fear that the *stillicidium* might remain; only if it is impossible to obtain a permanent cure by this means, the lachrymal sac should be obliterated. Dr. v. Gräfe communicates the following statistical results in reference to this operation:—Of one hundred patients in whom the lachrymal sac has been successfully destroyed, twenty suffer from permanent and troublesome overflowing of tears; seventy are molested neither at their work nor in the room, but experience increased moistening in open air, or if excited to tears, &c.; ten finally do not notice any difference from the normal eye. 3. In cases of caries, organic obstructions, &c., in which there is no prospect of restoration of the continuity, the lachrymal sac should be at once obliterated, as in any case the condition of the patient is ameliorated by this measure. Thus the troublesome suppuration is not only done away with, but some of the principal causes of the hypersecretion of tears are also removed, and in consequence of it the *stillicidium* is proportionately reduced.

ART. 92.—*A new treatment of Epiphora.*

By MR. J. V. SOLOMON, Surgeon to the Birmingham Eye Infirmary.

(*British Med. Journal*, March 12, 1859.)

This plan of treatment only differs from that recommended by Mr. Bowman in the mode of dividing the tear punctum and its canal. Mr. Solomon uses a pair of narrow-bladed scissors, instead of a grooved director and Beer's knife, and before making the incision he expands the punctum by the introduction of a probe or pin. Several cases are related in illustration.

ART. 93.—*Case of spontaneous Rupture of the Eye.*

By Dr. A. FLEMING.

(Amer. Jour. of Med. Science, April, 1858.)

CASE.—Reuben Pond, a sailor, æt. 50, was admitted into the Pennsylvania Hospital, September 22d, 1856. Two years previously, he lost the sight of the right eye, apparently from some inflammatory affection of the retina. Three days ago, this eye became the seat of painful throbbings, without any increase of bulk. On the day following, while occupied in some matters which frequently obliged him to hold his head in a dependent position, he was seized with acute pain in this eye, and very shortly he was alarmed by finding that blood was escaping in a distinct jet from it. The pain continued to increase in severity. On admission to the hospital, which was within a very short time after this accident, there was found to be a transverse tear of the cornea, from which arterial blood was escaping with considerable freedom. On introducing a probe through this opening, no trace could be found of the crystalline lens or its capsule; but in other respects the eye presented nothing at all remarkable in its appearance. The hæmorrhage was arrested by means of perchloride of iron and cold. Free suppuration followed, and the globe of the eye became considerably diminished in size, but eventually the tear in the cornea cicatrized, and on the 31st October the patient was able to leave the hospital.

ART. 94.—*Case of Pityriasis of the Eyelashes.*

By Dr. JAMES G. HELDIGE.

(Dublin Hospital Gazette, Jan. 1, 1859.)

CASE.—A weak, scrofulous-looking child, came to the Eye Dispensary, Mecklenburgh Street, complaining of inflammation of the eyelids. The tarsal edges were slightly ulcerated and inflamed, and the cilia were very long, and appeared of a shining brownish colour, and much thicker than natural. The itching was so intense that she was constantly rubbing the lids, which aggravated her symptoms very much. The only previous treatment had been bathing the eyes with an infusion of chamomile, which she did for four or five days, without deriving any benefit from it. On examining the lashes with a lens, Dr. Heldige detected an immense number of exceedingly small pediculi, about the size of the point of a pin, which, together with their ova, were exceedingly difficult to detach. They differed from the ordinary pediculi in shape and size, the legs being very long in proportion to the body, which was small, and of a circular form. The prescription was ointment composed of corrosive sublimate, 4 grs.; tar, ʒj; and lard, 3j; about the size of a pea to be rubbed into the tarsal edges of the lids morning and evening. This had the effect of completely removing the disease in about four days, and no relapse occurred during a period of five days longer that the case remained under observation.

This disease is not by any means of frequent occurrence. Mackenzie, in the last edition of his work, mentions only one case of it, and that not occurring in his own practice, but copied from a notice published in the 'Lancet' by Mr. Lawrence. Arlt, of Vienna, states that he has seen the disease occasionally; and Desmarres, of Paris, in his recent work, published in 1858, does not even allude to it.

The species of pediculi peculiar to the body and head are also sometimes found in the above situation; but those described by Mr. Lawrence, and observed in the above case by myself, are, according to my own observation peculiar to the eye-lashes.

ART. 95.—*On a new mode of operating for Cancer of the Lip.*

By Dr. O'SHAUGHNESSY.

(*Indian Annals of Medical Science*, July, 1859.)

Dr. O'Shaughnessy observes, that when cancer of the lip is confined to a limited spot, it is easily removed by the ordinary V-shaped incision, but that this procedure does not suffice when the whole lip, and perhaps one or both commissures, are involved in the disease. In a case which occurred in his own practice, the cancer not only occupied the whole lower lip, but the right commissure and a part of the upper lip also, on that side. "I thought nothing could be done in such a case, until the plan struck me of making a lip by detaching a triangular portion of the cheek on either side of the mouth, in the following manner: The whole of the diseased lip to be removed by making two incisions meeting at a point in the centre of the chin, the cheeks then to be divided by two horizontal incisions extending from the angle of the mouth on either side, and continued backwards as far as the masseter muscles; these to be joined, at their posterior extremities, by two oblique incisions carried upwards and backwards, from either side of the chin, leaving two triangular flaps to be dissected forwards, so as to admit of the apposition of the edges of the V-shaped gap left by the removal of the cancer." An excellent lip was in this way made, and most of the lines of incision had united by the first intention, when about the tenth day the patient was carried off from the effects of retention of urine.

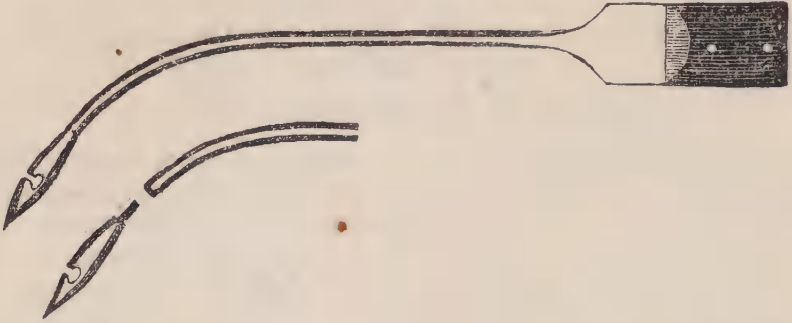
ART. 96.—*On a New Needle for the operation of Cleft-palate.* By CHRISTOPHER HEATH, ESQ., M.R.C.S., Demonstrator of Anatomy at the Westminster Hospital, and Surgeon to the St. George's and St. James's Dispensary.

(*Lancet*, Dec. 18, 1858.)

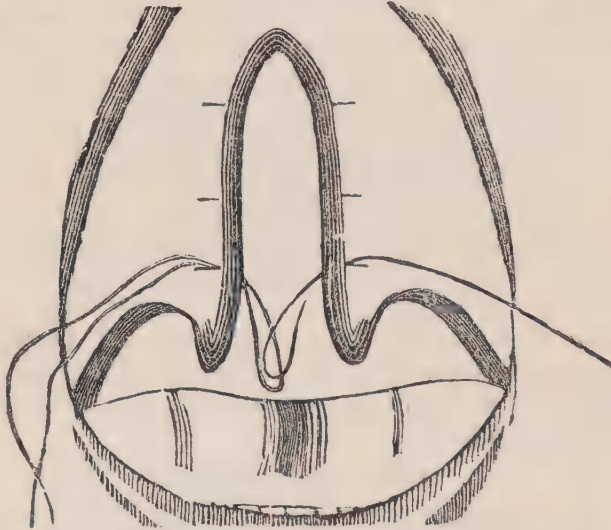
The operation of staphyloraphy seemed to have attained its perfection in the hands of Mr. Fergusson, who, after the division of the levatores palati, has been enabled to bring the sides of the cleft palate into apposition by means of silk threads, with unvarying success. The introduction of metallic sutures, however, into the practice of surgery, rendered some modification in the operation necessary, and accordingly, in the last four cases operated upon at King's College Hospital, Mr. Fergusson has knotted a silver wire to the thread previously passed across the gap, and has thus replaced the silken by a wire suture. The objection to this method of proceeding lies, first, in the extra complication, the next in the obstacle which the knot affords to the ready passage of the wire through the palate.

In the following modification of Mr. Fergusson's method, Mr. Heath

has endeavoured to avoid these disadvantages, and also the difficulty which is always experienced in seizing the minute thread at the bottom of the mouth, and drawing it out of the eye of the needle. The needle (fig. 1) employed (made by Matthews, of Portugal-street) differs from



the ordinary curved needles in being flattened in the opposite direction—*i.e.*, parallel to the plane of the curve, instead of at right angles to it. The head of the needle is also movable. The effect of this arrangement is, that the incision made by the needle is at right angles to the edge of the palate (fig. 2), instead of being parallel to it, and thus, apparently



there is less danger of the portion on which the thread rests sloughing away from want of nourishment. Besides, the head of the needle can much more readily be taken hold of by the forceps in this position and drawn out of the mouth, bringing the wire or thread with it, and thus avoiding the difficulty of catching the thread.

CASE.—Sophia S—, æt. 13, was born with a single hare-lip, and a cleft in the hard and soft palate continuous with it. The lip was operated upon successfully when she was an infant, but there being a very unsightly notch still present, I cut out the old cicatrix, and having pared the lip, brought the edges together, with great improvement to her appearance. The lip being perfectly healed, I proceeded to operate on the soft palate on the 15th October, 1858. Having divided the *lavatores palati*, and pared the edges of the

fissure, I proceeded to pass a single *freshly-annealed* silver wire with the needle I have described; this was easily drawn out of the mouth with the head, and detached through the slit in the eye, and I then passed a double loop of silk in the same manner on the opposite side. The end of the wire was now closely bent into the loop of the silk, and was thus readily drawn through the opposite side of the cleft (fig. 2). Two other sutures being introduced in the same way, the ends of each of the wires were simply twisted together, and held the edges of the palate in admirable apposition.

It may be thought there would be a danger of the head of the needle becoming detached, and so dropping into the patient's fauces; but it is effectually retained by the thread or wire being held tightly by the operator's fingers, and as the slit in the eye is on the convexity of the needle, there is no danger of the thread escaping through it prematurely. The use of the *freshly-annealed* wire was suggested to me at the time of the operation by my friend Mr. Barclay, and it is certainly preferable, from its greater pliability, to the hardened wire in common use. I need hardly say that it is prepared by heating the ordinary wire to redness, and allowing it to cool slowly.

The after-progress of the case was most satisfactory. The sutures were removed on the fourth and fifth days, when union was quite perfect.

ART. 97.—*On Laryngoscopy.* By Dr. CZERMAK, of Pest.

(*Wiener Wochenschr.*, No. 2, 1859; and *Edinburgh Medical Journal*, April, 1859.)

Under the head of a contribution to laryngoscopy, Dr. Czermak relates a case of chronic loss of voice, of supposed nervous origin, chiefly because it was often suddenly aggravated by emotional causes; in which, by means of a small laryngeal mirror and an ordinary study lamp, he was enabled distinctly to make out a small dark-coloured tumour, of the size of a small green pea, resting, by a tolerably broad base, on the right true vocal chord. The growth had a somewhat uneven surface, and seemed of soft consistence, inasmuch as each vibration of the chord caused its whole substance to tremble, and when closed on by the glottis, it seemed somewhat elastic; the sudden exacerbations of hoarseness, from emotional causes, depended, no doubt, on the more or less erectile character of the growth. Dr. Czermak only saw the patient once in passing, but suggests the propriety of operative interference in such cases, and relates this case chiefly as an encouragement to others to prosecute this method of diagnosis by means of Garcia's laryngeal speculum, recalling, however, the fact that this method of diagnosis dates long previous to Garcia, and referring for proof thereof to Liston's 'Practical Surgery,' London, 1840, p. 417.

ART. 98.—*Case of Polypus of the Œsophagus.*

By Professor A. H. MIDDELDORPFF, of Breslau.

(*Gaz. Hebd. de Méd. et Chir.*, Oct. 8, 1858.)

The following interesting case is from a monograph ('De Polypus Œsophagi,' Breslau, 1858) by Professor Middeldorpff, which contains a complete resumé of what is known in connexion with cases of the kind.

CASE.—Jos. Jaensch, born in 1811, lost accidentally, when young, the left eye, and a few years later, the left arm. He afterwards enjoyed good health, abstaining from excess of food and drink, and avoiding, particularly, irritating substances. In the spring of 1851 he contracted a violent catarrh, which rendered him deaf for a long time. A catarrhal inflammation of the pharynx remained, and embarrassed deglutition, particularly that of dry bread; excretion of pituitous liquid between the meals; sensations of pressure at the epigastrium and behind the sternum; painless dysphagia, but gradually increasing, very marked in certain positions of the patient, and accompanied by eructation, cough, and dyspnœa; the difficulty of deglutition daily augmented; finally, the ingestion of liquid aliments alone possible. During an attack of violent cough, the patient once vomited sanguinolent mucus.

At the end of the year 1852, the trouble of deglutition having reached its intensity, he drank water copiously, and was seized with violent vomiting, during which a body resembling a kidney was raised into the mouth between the teeth. An intense dyspnœa followed. The patient succeeded in swallowing the body in question, and applied for medical aid. When Professor Middeldorpff first saw the patient he was pale and much emaciated; the finger, introduced into the pharynx to the posterior wall of the larynx, did not come in contact with any foreign body; a probe, of half a finger's diameter, was passed down in the median line, and in directing it to the right side, an obstacle was found, which could be easily overcome, and was not very deeply seated.

On the 14th of January, 1853, Professor Middeldorpff directed the patient to take a large quantity of warm water and an emetic. During the violent vomiting which ensued, a turgid and violaceous body appeared between the dental arches—this was the polypus. Professor Middeldorpff seized it with the forceps of Museux, drew it toward the left commissure of the mouth, in consequence of which the difficulty of respiration was somewhat diminished; a ligature of waxed silk-thread was applied over the tumour, at a level with the base of the tongue. The operation was attended with repeated vomitings and great dyspnœa. The polypus was afterwards divided with the scalpel three quarters of an inch above the ligature; it became turgid, of purple colour, and a great quantity of blood escaped from it; in a short time its colour sensibly diminished. The patient afterwards swallowed the pedicle of the tumour and the ligature, the extremities of which were attached around the left ear; the vomiting, oppression, and dyspnœa ceased immediately; the patient felt very comfortable; it was ascertained, on repeated trial, that slight traction at the end of the ligature produced pain. The patient was ordered not to touch the thread, to use fluid and cold food, and to present himself every second day. Nothing remarkable occurred up to the eighteenth day after the extirpation. At this time the loop of the ligature rose into the mouth; it measured twelve millimètres in diameter, and thus encircled a pedicle of about thirty-seven millimètres in circumference. From this moment the troubles of the patient ceased; deglutition and respiration were unembarrassed; the appetite soon revived, and the patient gained in strength and flesh.

Professor Middeldorpff saw his patient five years after the operation; his health was then excellent; the excretion of pituitous liquid had ceased. On external examination of the neck nothing abnormal was discoverable; a probe of eight lines diameter still met with a slight obstacle at a level with the larynx; in short, the health of the patient was perfectly established.

Examination of the tumour.—The excised tumour measured eight centi-

mètres in length and four in thickness, and weighed about forty grammes; it was cylindrical; its smooth and shining surface presented some inequalities and excoriations which bled easily; it was covered externally by a layer of stratified pavement epithelium; underneath this a layer of conical papillæ was detected; the papillæ were visible to the naked eye, and arranged with great regularity in a spiral around the longitudinal axis of the tumour, and received vessels at their base; underneath the papillary layer the proper substance of the tumour was situated; Professor Reichert found it, on examination, very vascular, but without nerves; it was composed of connective tissue, in which the cellular elements were still very visible: in other words, a connective tissue not yet arrived at maturity; here and there fat-globules, either free or in cells, were detected; it was, in short, a vascular fibrous tumour, covered with papillæ. By comparing the length of the extirpated portion with that of the remaining portion of the polypus, and by making repeated measurements and explorations in the dead body, Professor Middeldorpff arrived at the conclusion, that the extremity of the polypus was situated about two inches from the carotid, and that the pedicle was attached at about a level with the larynx. Although these data are merely approximative, they harmonise, in a remarkable manner, with the results furnished by the catheterism.

ART. 99.—Case of Dislocation of the Fourth and Fifth Cervical Vertebrae.
By Dr. W. M. RYER.

(*Southern Medical and Surgical Journal*, Feb., 1859.)

CASE.—The subject of this accident was a girl, seven years of age, of lymphatic constitution, the daughter of Dr. Hepburn, of Mokelumne Hill. When seen by Dr. Ryer, the patient's head "was most singularly and immovably fixed, much bent to the side, the ear approximating but little in advance of the right shoulder, and in a position no child in a normal condition could for a moment assume; the slightest motion tending to change the relative position of the head and body producing intense pain.

"The father, Dr. Hepburn, an aged and very intelligent practitioner of medicine, had watched the child with a parent's solicitude for the six previous days and nights, and neither during sleeping or waking did the child move its head from the position it had assumed from the instant of the accident. As the right clavicle was fractured at the time, the doctor was inclined at first to believe the child was favouring the fracture and was unwilling to entertain the unpleasant thought of so serious a complication as luxation of the spine.

"The child had fallen six days previous to my visit, from a high bed, and is supposed to have struck the back and left side of her head. The father saw her within a half minute after, and found her head and neck distorted precisely as at the time of my examination; there had been no change for six days. Such distortion, I believed, must have arisen from muscular contraction or bony displacement. We examined every muscle whose contraction would be likely to produce the deformity, and found them loose, soft, and uncontracted. Upon tracing the spinous processes from below to the articulation of the fourth and fifth cervical vertebrae we found them form, at this point, an obtuse angle, and depart from the natural direction about forty degrees. The intellectual faculties were good, and sensation and motion not greatly impaired. I could form no other diagnosis than was formed

by the medical gentlemen in attendance previous to my visit. It was clearly a dislocation of the left oblique articulating process—the process of the fourth riding over the upper margin of the one with which it was articulated below.

“As objections were made to the administration of chloroform, we attempted the reduction without it, and failed. We then administered this anæsthetic and succeeded to our fullest anticipations, Dr. Soher, and other gentlemen who assisted, distinctly recognising the instant of time when the reduction was effected. The child immediately had full motion of her head and neck, and is now entirely recovered.”

(B) CONCERNING THE CHEST, ABDOMEN, AND PELVIS.

ART. 100.—*A rare form of fracture of the Clavicle.* By M. ROBERT.

(*L'Union Médicale*, No. 79, 1858; *Med.-Chir. Review*, Oct., 1858.)

In the adult fractures of the clavicle are almost always oblique, the periosteum being torn and the fragments displaced. Sometimes, however, in the adult, but oftener in the child, the fracture may be transverse, without laceration of the periosteum or displacement. This was the case with a miserable-looking lad, aged sixteen, brought to M. Robert at the Hôtel Dieu, his left clavicle having come in contact with a table during a fall. There was no displacement, and the bone presented quite its normal appearance, there being neither ecchymosis, projection, nor depression. On passing the fingers along it, however, with a gentle pressure, a painful spot, with a slight mobility at it, was felt; and whenever the long thin bones—such as the fibula, ribs, or clavicle—have been exposed to violence, and no deformity is observed, pain limited to a very small portion of their course is quite sufficient to give rise to the belief in the existence of a fracture. To search here for crepitation would only rupture the periosteum still entire, and thus increase the gravity of the case. All that was required was to keep the arm against the chest, and caution the patient against using it—a caution of importance, for there being no displacement and but little pain, the patient, believing the bone not broken, might easily employ the limb dangerously.

In a few days the tissue surrounding the fractured point became inflamed, and the swollen periosteum formed a projecting ring uniting and maintaining the fragments in contact; confirming entirely the diagnosis derived from the localized pain and slight mobility first observed. Cases come to the hospital a week or a fortnight after the accident, exhibiting merely this circular ring, the nature of which may be yet more difficult to understand, as the patients have often forgotten that a fall or blow has taken place. It is well to be aware of this, as the projection may be mistaken for a periostitis or an exostosis. It is easily ascertained to be a ring constituting a provisional callus, by imparting to the bone movements which are easily perceived, the ring being as yet only in its fibrous or cartilaginous condition. It gradually ossifies, and is replaced by a small definitive callus. Ordinary fractures of the clavicle are hardly ever accompanied by a provisional callus, this being only found when the periosteum is preserved entire.

ART. 101.—*On some undescribed affections of the Spinal Column, the result of pressure.* By Dr. J. H. PACKARD, of Philadelphia.

(*American Journal of Medical Science*, Jan., 1859.)

There is a result of indirect violence not mentioned in any systematic work, which is nevertheless of some importance. This is produced by powerful pressure at or near each extremity of the vertebral column.

"A coal-miner," writes Dr. Packard, "was sitting upon a large piece of coal, and bending forward to his work, when a mass was detached just over his head, and came down upon him. The force thus brought to bear was immense, and its results were in proportion; the sacrum was fractured transversely as well as longitudinally, and its lower extremity was comminuted, as was also the coccyx; the right sacro-iliac symphysis was forced open; the horizontal ramus of the pubis of each side, and the ascending ramus of the ischium of each side, were fractured. There was also a fracture of the left tibia, and a complete rupture of the urethra.

"When force of this kind acts upon the true vertebræ only, it may have a very singular effect. A young man, 17 years of age, was admitted into Pennsylvania Hospital, in September, 1855; he had been sitting upon a log, beneath a staging upon which there were a good many people, when the staging gave way. His spinal column had thus been subjected to great force at each extremity. When brought to the hospital, he was much collapsed, and suffering extreme pain; his back presented a striking prominence at about the eleventh dorsal vertebra. The pain extended all around his body; neither the sensibility nor the motions of his lower limbs were impaired. He was laid in bed upon his right side; reaction having occurred, counter-irritation and diaphoretics were employed, and his bladder emptied once by means of a catheter. No bad symptom ensued; a week after his accident, he was able to stand up, holding by a chair; and he gradually gained strength, although the deformity of his back remained. At the end of six weeks he was well enough to be discharged.

"Now, what was the lesion in this case? The eleventh dorsal vertebra formed a very marked projection backwards; or, to speak more correctly, the spinous process of that bone constituted the apex of the angle made between the upper and lower portions of the vertebral column. There could not have been any great degree of compression of the cord, without some symptoms; but such compression would seem inevitable, if luxation had been present. Nor is it at all certain that luxation can occur in any but the cervical vertebræ; no instance is recorded in proof of such a possibility.

"The supposition of a fracture was excluded by the impossibility of lessening the angular bending of the spine, by the want of crepitus, and by the rapid recovery; and besides, had such deformity been the result of fracture, injury to the cord would most certainly have been sustained.

"Sir Astley Cooper relates a very similar case to the above, except that in it there were two or three spinous processes broken also, and a laceration of the muscles on one side; complete recovery ensued.

"It was suggested by a gentleman who saw the case of which I have given the details, that the injury was neither a fracture nor a luxation,

but a squeezing out forwards of the inter-vertebral substance, the mechanism being the same as when the body of a vertebra is crushed by indirect violence. The inter-vertebral fibro-cartilage is held in place by very close and strong attachments to the bones above and below; it is, moreover, confined on every side by the ligaments, and especially in front by that one which usually receives the name of *anterior common* ligament; so that such a displacement of it would seem almost impossible practically. This explanation must, therefore, be looked upon as purely theoretical, until an opportunity occurs for verifying it by dissection.

“Two cases of somewhat similar deformity have come under my notice, although, as will be seen, their attendant circumstances were different; I call them similar, because they likewise concerned the eleventh dorsal vertebra, which was abnormally prominent, and because the precise nature of the lesion could not be detected. In one of them, the child was a stout and healthy girl, four years of age; five or six weeks previously to my seeing her, she was known to have fallen down some steps. When her mother brought her to me, she told me that she had noticed, a few days before, something peculiar in her walk, and was led to examine her, when she found a lump in her back. This lump proved to be the spine of the eleventh dorsal vertebra, projecting very slightly to the left, and maintaining perfectly its relation to the transverse processes and to the ribs; as if the lower part of the spinal column had been displaced forward *en totalité*. The child walked quite feebly, and carried its shoulders a good deal backward. No effect had been produced upon the bladder or rectum. Unfortunately, this very interesting case has passed beyond my reach.

“The other case was that of a girl $3\frac{1}{2}$ years old, not at all healthy in appearance, and of small stature. About a year before she was brought to me she had a very serious illness of some kind, and never perfectly recovered from it; at about the same time she fell down some stairs, and to this fall her mother seemed disposed to attribute the affection of her spine. She began to be quite lame about six months afterwards. Upon examination, three months ago, the eleventh dorsal vertebra was seen to project backward to a marked degree, much as in the preceding case; and this child, like the other, walked feebly, and carried its shoulders a good deal back. There was, however, some difficulty in urination in this case, and the right lower extremity seemed shortened. An accurate investigation was almost impossible, from the extreme fretfulness of the child. A stimulating liniment was ordered, with tonics, and a simple but nutritious diet; but, as might have been predicted, no change has taken place in regard to the local affection.

“Now, in these two children, fracture may be at once excluded from consideration in making a diagnosis, for obvious reasons. Luxation seems equally improbable. Might there have been a displacement, partial or complete, of the inter-vertebral substance, or possibly a destruction of it by disease? And if so, how are we to explain the peculiar deformity, and the carrying back of the shoulders? The recent date of the first case, and the robust health of the child, excluded from my mind the idea of disease of the bones or fibro-cartilages; while in neither was there the tenderness on pressure, to say nothing of the symptoms connected with the spinal marrow, which usually accompany such affections.

“To explain the cases now related, it seems to me that we must assume the possibility of some as yet undescribed lesion of the vertebral column; that neither luxation, sub-luxation, nor fracture could have existed without symptoms quite different in degree, if not in kind. The exact nature of this lesion will probably remain obscure until an opportunity occurs for studying it by dissection.”

ART. 102.—*Fracture of the tenth Dorsal Vertebra, and elevation of the depressed portion.* By Dr. STEPHEN SMITH, Surgeon to the Bellevue Hospital, New York.

(*New York Jour. of Medicine*, Jan., 1859.)

CASE.—David King, derrick-man, born in Ireland; æt. 41; temperate; of good constitution, and of robust appearance; large frame, and well nourished. Admitted October 12th, p.m.

History.—Two hours previous, when riding up 9th Avenue, he was thrown from a cart, and struck in some manner upon his back; was not rendered unconscious; did not feel hurt until some one attempted to take him up; then found that he was paralysed, and that the motion caused him intense pain. Could not tell whether the injury he had received was from striking upon some projecting point, or indirectly, by force transmitted from a blow received elsewhere.

Examination.—When admitted he was collapsed; pulse too frequent and feeble to be counted; respiration 18 in the minute. Paralysis and anæsthesia of the entire body was complete below the sixth intercostal space. There was moderate priapism. The temperature of the body was normal. The only subjective symptom was severe pain in the back of the neck, and pain, numbness, and tingling in the arms. Upon examination, a space between two of the spinous processes of the dorsal vertebræ was detected, in which two fingers could be placed; no corresponding superficial mark of injury was visible. Nothing abnormal could be discovered in the cervical region. Though motion was productive of severe pain in the back, the pain did not appear to be so excruciating as frequently happens in spinal injury. The urine was drawn by catheter, and a free administration of stimulants and an anodyne ordered.

Progress of case.—Oct. 13th, a.m. Pulse 112, and of considerable force; respiration 26 and mainly abdominal; the temperature of the body and of the lower extremities was exalted; anæsthesia had extended slightly upward upon the body to the fifth rib, and the pain and numbness of the neck and arms had decidedly increased. The penis was not erect, but very easily excited on irritating the spine. The urine was again drawn by catheter, and the treatment of last night continued. P.M.—Sloughs had commenced upon the heel, and upon the ball of the great toe of the left foot, and over the external malleolus of the right ankle. Seen by Drs. Stephen Smith, J. R. Wood, and Crane, and in consultation immediate trephining of the vertebral column was determined on, as affording a possible chance for the patient's recovery.

Operation.—The operation was performed in the ward by Dr. Stephen Smith. Chloroform was administered, and an incision of six inches in length made over the spinous processes of the dorsal region. An extravasation of blood nearly the length of the incision was found in the subcutaneous tissue, and a depression of a lamina upon the right side of one of the lower

dorsal vertebra detected. An unsuccessful attempt was made to divide the lamina upon the opposite side by a rotatory saw; the trephine was then applied to the same, and afterwards Hey's saw. The depressed portion of lamina was pulled out by a duck-billed forceps, and through the opening thus made flowed from six to twelve ounces of dark, extravasated blood.

The patient was entirely under the influence of chloroform during the operation, lying upon the side; respiration free, and all the time satisfactory. No benefit resulted from the operation, and death occurred soon after.

Autopsy.—Fracture of the body of the tenth dorsal vertebra upon the right side, extending from the base of the transverse process half way to the mesian line anteriorly, without displacement—fracture of the arch of the vertebra upon the right side with depression; and extravasation of blood to a large amount, extending from the lower cervical vertebra to the sacrum. Considering the increasing paralysis, this extravasation was probably still extending upward at the time of the death of the subject.

ART. 103.—*On the diagnosis of the Sacro-iliac Disease.*

By Mr. ERICHSEN, Professor of Surgery in University College, London.

(*Lancet*, Jan. 8, 1859.)

Mr. Erichsen gives the names of *sacro-iliac disease* to disease of the articulation between the sacrum and the pelvis. The diagnosis, he tells us, is not always easy, and he points out five affections with which it may be confounded—namely, neuralgia of the hip, sciatica, spinal disease, coxalgia, and disease of the pelvic bones.

1st. *Neuralgia of the hip* in young females may readily enough be confounded with the earlier stages of sacro-iliac disease. But the widely-spread and superficial nature of the pain in the neuralgic affection, the co-existence of the hysterical temperament, the sex of the patient, and the absence of all limitation of morbid action to the neighbourhood of the diseased articulation, render the true nature of the affection sufficiently clear. The obliquity of the pelvis, which occasionally occurs in neuralgia of the hip, and causes apparent elongation of the limb, is readily removed when the patient lies on her back; whereas, in sacro-iliac disease, position does not affect the displacement of the limb on the affected side.

2d. *Sciatica.*—In this affection, the age of the patient, usually more advanced than that of the subjects of sacro-iliac disease; the seat of the pain, below the articulation, and its extent down the back of the limb; with the absence of elongation, will enable the surgeon to affect the diagnosis.

3d. From *spinal disease*, the diagnosis is usually sufficiently easy, for although the situation of abscess resulting from caries of the vertebræ may in many cases be the same as that which is occupied by the collections of pus resulting from "sacro-iliac disease," yet in caries of the spine, in the vast majority of instances, excurvation of the vertebræ has become prominently marked by the time that the abscess has assumed so great a magnitude as to occupy the inferior lumbar or gluteal regions. In those rare cases in which, as an instance that was recently under my care, caries of the vertebræ, with consecutive abscess, takes place with-

out any angular curvature, it will be found that the patient complains of tenderness on the surgeon percussing the spine opposite the seat of disease; that the spinal column has lost its flexibility, moving stiffly and as a whole; that there is an absence of that elongation of the limb on the affected side, dependent on displacement of the wing of the pelvis, which is so early observable in sacro-iliac disease; and lastly, that examination of the sacro-iliac synchondrosis neither elicits pain nor reveals swelling or any of the other signs of disorganization of that articulation.

4th. *Coxalgia* is the affection that is most easily confounded with sacro-iliac disease, and that from which it is of most importance to make the diagnosis. It is especially from that variety of hip-disease that commences in the acetabulum, that primarily involves the pelvic bones, and only secondarily implicates the joint, that it is difficult to distinguish sacro-iliac disease; and the importance of affecting this diagnosis is great when we reflect that these cases of hip-disease may now successfully be subjected to operative interference, whilst sacro-iliac disease does not admit of relief or removal by these means. Now, the diagnosis between coxalgia in all its forms and the disease we are at present considering may be effected by attention to the following circumstances:

(1.) The seat of pain on pressure. In hip-disease the patient suffers most severely when pressure is exercised deeply behind and above the trochanter, in the hollow behind that osseous prominence, or when the compression is exercised against the anterior part of the hip-joint through the pectineus muscle. In sacro-iliac disease little or no pain on pressure is experienced in these situations; but tenderness is elicited by pressure upon the sacrum and along the line of junction between the sacrum and ilium, behind and altogether away from the hip.

(2.) The movements that occasion pain are different in the two diseases. In hip disease, abduction and rotation outwards, or pressure of the head of the femur into the acetabulum, aggravate, to a greater or less degree, often to an unbearable extent, the sufferings of the patient. In sacro-iliac disease the thigh may be moved in all directions, ab- or adducted, rotated, flexed, or extended, whilst the patient is lying on her back, without any increase of suffering, *provided the side of the pelvis be fixed by the surgeon*. Should this precaution not be taken, the movement impressed on the thigh will be communicated to the diseased articulation and will necessarily occasion suffering.

(3.) The signs connected with the alteration in the length of the limb differ in the two diseases. In hip disease there may be, and usually is, in the advanced stages, considerable shortening. This never occurs in sacro-iliac disease. In the earlier stages of coxalgia there may be, as there is throughout in sacro-iliac disease, elongation of the limb. But there is an important point connected with this. The elongation in hip disease is always appreciable by measuring from the anterior superior spine of the ilium to the inner ankle. In sacro-iliac disease, however, the measurement between these two points on the opposite sides of the body exactly correspond, the seat of the elongation being situated still higher up.

(4.) The alteration of the level and of the prominence of the two anterior superior spines, in sacro-iliac disease, may be confounded with that

arising from the obliquity of the pelvis usually occurring in the early stages of coxalgia. But here also the diagnosis may be effected by observing that the displacement of the bone in sacro-iliac disease is permanent, and is not influenced by position. The obliquity of the pelvis in hip disease, giving rise to apparent elongation of the limb, is dependent on a twist in the lumbar spine, which may be rectified by placing the patient on his back, and using a little manipulation. The alteration in the level of the two ilia, in sacro-iliac disease, is not modified by change of position, or by any movement that may be impressed upon the spine.

5th. *Disease of the pelvic bones* may of course occur independently of any affection of the sacro-iliac articulation, and when so occurring, it always commences at a distance from it, the crista ilii, the tuber ischii, or the acetabulum, being the usual seat of the disease. When occurring in the first of these two situations, the resulting abscess seldom attains a very large size, and is altogether above or below the synchondrosis, the outline of which can be felt clear and unobscured by swelling of any kind. When the abscesses are opened, the sinuses that result will lead directly down to the rough and carious bone, examination of which will leave no doubt as to the nature of the cases. In these cases, also, no change takes place in the length of the limb, or in the position of the side of the ilium.

When the acetabulum is primarily affected, the difficulty of diagnosis may be greater, in consequence of the large size and often intra-pelvic nature of the abscesses, and the co-existence of a certain amount of displacement or elongation of the limb. But here the same circumstances that enable the surgeon to effect a diagnosis in ordinary coxalgia—viz., the pain in movement influencing the hip-joint merely, and the increased length of limb, as determined on measuring from the anterior superior spines—will prevent his falling into error as to the true nature of this disease.

ART. 104.—*Displacement of the Coccyx sideways.* By Dr. ROESER.

(*Prager Vierteljahrschr.*, No. 18, 1858; and *Edinburgh Medical Journal*, Nov., 1859.)

CASE.—“A corpulent woman, æt. 36, fell from a table on a chair, so that its back came right between her thighs. She instantly felt severe pain in the coccyx, but continued able to move about till evening, sitting increasing the pain very much. In the evening the pain was so great, extending up the spine, that she was obliged to go to bed, and soon after could neither turn nor rise up. After a painful night, R— found this otherwise blooming woman quite immoveable, with distorted features; she complained of violent pain in the coccyx, and a painful tensive drawing feeling from below, up to the neck, which also extended down the arm. She could move the forearm a little. The slightest motion of the body or head to one side was impossible, and still more so sitting up in bed: confused headache, and some mental disturbance were also present. She made no complaints of her lower extremities, nor of her arms, and urinated without difficulty. After placing her on her right side, a small swelling, the size of a hazel-nut, was felt near the notch of the buttocks next the left ischium, which, on closer examination, proved to be the coccyx separated from the sacrum, and forced from the mesian line towards the left ascending ramus of the ischium. The obtuse

end of the sacrum could easily be felt between the buttocks. By placing one finger in the rectum the dislocation of the coccyx could be still more easily felt; forcible pressure downwards and towards the right buttock caused it suddenly to glide into its normal position, whereupon the patient declared herself relieved, herself as if roused from a dream, and all her pains vanished. She could move about freely; but pain in the sacro-coccygeal region prevented her sitting up; her expression was also completely changed. After a few days, a dull pain in the sacro-coccygeal region preventing sitting, was all the uneasiness that remained; and in five days, all of this that was left was a slight burning sensation at the injured spot." The irritation of the spinal marrow observed in this case, in which only the very lowest filaments could have been disturbed, and which nevertheless sent the stream of disturbance to the brain itself, is a most interesting example of mechanical irritation, as evidenced by its instant disappearance on the reduction of the dislocation.

ART. 105.—*Empyema presenting in the lumbar region, and deriving pulsation from the neighbouring arteries.* By Dr. G. OWEN REES, Physician to Guy's Hospital, &c.

(*British Med. Journal*, Aug. 21, 1858.)

In this very unusual case a collection of fluid, originating, no doubt, in empyema of the left pleural cavity, appeared in the loin, as a pulsating aneurism-like tumour.

CASE.—Daniel Brooks, æt. 9. He had enjoyed good health until about eleven months ago, when he was admitted into Guy's Hospital, under Dr. Hughes, with what appears, from the record of his case, a decided attack of pleurisy, with inflammatory effusion on the left side, the symptoms of which soon subsided, and he was in a few weeks enabled to return to school. Three weeks ago, whilst she was washing him, his mother observed a swelling in the lower part of the back; this has given no pain either before or since its discovery, and he is not aware whether or not it has increased in size.

Present condition.—He is a fair-complexioned boy, spare and of delicate aspect, and of somewhat more than average intellect. The chest is prominent in front; and rather deep from before to behind; the lower true and false ribs are more prominent on the left than the right side, with the intercostal spaces pressed out on a level with them. On the right side, the ribs rise and fall naturally; there is resonance to percussion; the vesicular murmur is healthy, almost puerile; the left side rises but little on deep inspiration; there is total absence of vesicular breathing throughout the lower half of that side, and no vocal fremitus. The heart occupies the centre of the chest; its pulsation is evidently below the ensiform cartilage; at this point the sounds, which are natural, are most distinct. The abdomen is soft and yielding. On the left side of the spine, above the crest of the ilium, is a tumour, rounded in shape, with a diameter of two and a half to three inches. It conveys to the touch a sense of fluctuation; the fluid is apparently not deep seated; there is no pain, redness, or heat of the integument covering it; and, on applying the stethoscope, a distinct pulsation is experienced which is synchronous with the arterial pulse. There is no tenderness or irregularity in the course of the vertebral column. The tongue is very slightly furred,

red at the tip and edges. The appetite is good; the bowels are daily relieved; the urine is healthy.

January 14th.—He was ordered to have half an ounce of cod-liver oil three times a day, and to have a linseed-meal poultice applied to the loins; and middle diet.

18th.—The tumour was slightly increased in size.

21st.—The swelling now visibly rises with each systole of the heart's action. There is now a little pain produced by pressure.

28th.—The wall of the tumour is becoming thinner, the skin much discoloured. He suffers hardly any constitutional symptoms, and has somewhat improved in appearance since admission.

February 2d.—In consequence of the oil producing nausea, he was ordered ten minims of tincture of sesquichloride of iron, in infusion of calumba, three times a day.

9th.—The tumour has now attained the size of a large orange; the pulsation is very strongly marked. The patient's health seems hardly so good as formerly.

11th.—The assistant-surgeon, Mr. Bryant, to-day explored the tumour with a fine trocar and canula; and pus having been discovered, it was opened with a lancet, a few ounces of pus evacuated, and a poultice applied. On measuring the circumference of the chest over the lower part of the ribs, it is found that that side which appeared the largest, from the ribs being pushed out, is in truth an inch less than the healthy side.

13th.—The free discharge of the contents of the abscess is somewhat prevented by the opening becoming obstructed by flakes of corpuscular lymph. The pulse is small and weak, 120.

15th.—This morning the wound, having for a short time been obstructed, reopened, and a large quantity of healthy pus, amounting to nearly two pints, was evacuated. He still suffers but slightly in general health, and complains of no pain. Ordered to take a chop daily, and three ounces of wine.

24th.—The abscess still discharges freely. Poultices are still applied. The prominence of the left side of the chest is much less marked; and a decided lateral curve exists in the spine, the convexity looking towards the healthy side (right).

March 6th.—There is now a considerable amount of curvature of the spine towards the right side, producing a striking alteration in the conformation of the chest, compared with the appearance presented on admission; the *right* side of the chest being now very prominent, but perfectly resonant, while the left is flattened, but remarkably dull. The impulse of the heart is best felt just to the left side of the lower end of the sternum, the apex being apparently tilted up more than formerly, in consequence of the subsequent curvature. The abscess is still discharging very freely; the patient's health begins to suffer; pulse 110; tongue clean, appetite moderate; with frequent flushings of the face.

13th.—The hectic fever is now strongly marked, and the patient is evidently losing flesh. He has again unsuccessfully attempted cod-liver oil, and is now taking half a drachm of syrup of iodide of iron, in equal parts of infusion of quassia and calumba, three times a day. The walls of the abscess are somewhat contracted; the left side of the chest is still very dull; yet the neighbouring respiration is much more evident; bronchophony is heard lower down, and, at the apex of the lung, puerile breathing.

19th.—Harsh respiration may now be heard over almost the whole of the left lung, and loud ægophony; there is still a good deal of dulness; he is

certainly losing flesh. He is ordered to continue the mixture, and, in addition, to take two drachms of glycerine three times a day.

30th.—He perspires profusely. From this time he fell into a hectic condition, and continued to sink, without any symptoms calling for particular note.

April 20th.—Slight convulsions have at intervals occurred, and partial paralysis of the right side is evident. He is quite unconscious.

21st.—He died.

Autopsy, six hours after death.—The body was extremely wasted. The surface of the brain presented the appearance, not of acute disease, but of chronic wasting. The subarachnoid spaces contained a large quantity of serous fluid. On the sides, a small quantity of purulent fluid was seen, and a few tubercles. At the base was a small quantity of tough lymph, with tubercles; and in the fissure of Sylvius, on each side, the tubercles were in great abundance; they were numerous, also, in other parts. The lateral ventricles contained an increased quantity of fluid; the surface of them, as well as of the fourth ventricle, was granular all over. The left lung was contracted to a small space, and occupied the upper part of the chest. The lower half of the chest constituted an abscess with the diaphragm below: a long sinuous opening passed behind this muscle for about six inches to the loin below, where it was emptying itself. The left lung contained tubercles at the upper part, whilst the lower lobe of the right lung was consolidated by red hepatization. The heart, with its lining membrane, was healthy; as likewise the abdominal viscera. Not a trace of tubercle could be discovered in the peritoneum.

ART. 106.—*A watery collection in the abdominal parietes simulating Ascites.* By Dr. FRÖLICH.

(*Prager Vierteljahrsch.*, t. iv, 1858, and *Archiv. Gén. de Méd.*, Jan. 1859.)

CASE.—A woman, whose abdomen had rapidly increased in size, and presented all the ordinary signs of ascites. Paracentesis was performed in 1847, and a considerable quantity of serum evacuated. After this the fluid accumulated again, and the patient sank from exhaustion in 1858. On examination after death, there was an enormous quantity of fetid, turbid, flocculent fluid between the integument and the abdominal muscles, and at the bottom of the cavity two cherry stones. The third portion of the duodenum was adherent to the peritoneum, and the cavity of the bowel communicated with that in the abdominal parietes by four holes of about the size of peas. The other intestines were mutually adherent in several places.

ART. 107.—*On the radical cure of Hernia by the metallic seton.* By (1) Mr. REDFERN DAVIES, Surgeon to the Birmingham Workhouse Infirmary, and (2) Mr. T. SPENCER WELLS, Surgeon to the Samaritan Hospital.

(*Med. Times and Gaz.*, Feb. 5, and Feb. 12, 1859.)

The idea of treating hernia in this manner appears to have occurred to these two surgeons almost simultaneously. Mr. Redfern Davies gives four cases of femoral hernia, and two cases of ventral hernia, in which the opera-

tion was carried out very successfully. He also refers to more than thirty other cases upon which he has operated, and of which he proposes to give the history on another occasion. Mr. Wells gives only one case, and that one in which Wützer's operation had been tried previously with only partial success. It does not appear, indeed, that Mr. Wells is disposed to propose this operation as a substitute for Wützer's operation, which operation, he says, "is so very safe and successful in the cases to which it is applicable, that I do not think we can improve upon it."

1. *Mr. Redfern Davies' Cases.*

Thomas M—, æt. 20, had been the subject of a femoral hernia, the result of a strain, for two years; it protruded constantly; in size about that of a pigeon's egg, and on account of its painfulness prevented him working. Accordingly, the patient on his back and the buttocks well raised, the hernia was returned, the little finger carried well through the crural canal and femoral ring, the artery being plainly felt beating on the outside. A well-curved canula needle was carried on the finger, and its point made to press against the abdominal parietes; the needle was then thrust through them, and a silver wire being passed into the eye it was drawn through. A small piece of vulcanized india-rubber, about the size of a split pea, attached to the crural end, and fastened by means of a split shot; the other extremity was then passed through another piece of rubber about the size of a florin, the two extremities drawn tightly together and fastened by a split shot. A portion of integument was thus drawn into the canal.

The wire was loosened on the seventh day, and finally removed on the ninth. There was a copious discharge of a very thick yellowish green pus, in the track of the wire, and around considerable thickening from effusion into the tissues.

That portion of the wire which was inside the body, *i. e.* between the points of exit and entrance, was quite bright; water-dressing and compress applied.

Upon examination a few days ago, by some of my professional friends, he was found radically cured.

The next case was that of a woman, which is so similar that I therefore do not detail it.

The third case, Amelia M—, æt. 50, has been ruptured for three years. The hernia was about as large as an orange, and the femoral ring would admit a man's thumb. For two days she went on very well, and had her bowels opened well once. On the third day she complained of pain in the abdomen, chiefly at the epigastrium; firm pressure around the seat of operation was not felt. In the evening there was a tympanitic condition of the abdomen on percussion; she said she should be better if her bowels were opened, and accordingly she had a dose of castor-oil and tr. of assafœtida. In the morning, bowels not opened; tympany increased; pulse weak; tongue dry; skin dry; and she vomits everything. No pain around the seat of operation, and no inflammation can be seen there. She was ordered ice, brandy, and injections of castor-oil, turpentine, and tr. assafœt.; also croton oil by the mouth; a long tube was passed per rectum, but only a little flatus escaped. In the evening bowels not opened; injections coming away unaltered; stercoraceous vomiting, and much troubled with wind on the stomach. In short, on the fifth day she died.

Post-mortem examination.—Abdomen swelled and tympanitic. Upon being opened, a considerable escape of gas; great omentum injected.

Upon carefully examining and removing the intestines, they were found to

be injected, but otherwise healthy in appearance, till the junction of the ileum and cæcum was arrived at; this portion was found bound down by old adhesions, which, being separated, an abundant escape of pus took place. Near the ilio-cæcal valve was an ulcer of about the size of a shilling, its edges raised, indurated and ragged, the surface depressed, and presenting a perforation of about the size of a split pea. The surface of this ulcer looked to the fascia beneath, and, by adhesions before spoken of, was attached to it, preventing the escape of fæces into the peritoneum.

The ilio-cæcal valve itself presented usual appearances, but at the aperture between the ileum and cæcum was a hard flattened piece of fæces, blocking up the communication.

The portion of the external surface of the abdomen, comprising the operation, presented the three apertures of exit of the wires—through whose track a little healthy pus was exuding, and around each was some thickening of the tissues. On the visceral surface of the abdomen the peritoneum was in a perfectly normal state, no sign or trace of any inflammation-action being present in it. Each puncture was cleanly cut, and presented no inflammatory action. That portion of the silver wire which was contained in the body was quite bright and unaltered. The conclusion we came to was, that the cause of death was the perforating ulcer of colon, and that the hernia was, as far as it went, in all respects, as well as could have been wished.

Alice L—, æt. 50, has been ruptured on both sides for nearly twenty years. The femoral rings are very large, admitting about two fingers. The ruptures extend to the middle third of the thigh, in size about that of a man's head. They are easily returned, but no instrument or apparatus will retain them, consequently she can only lie and sit, locomotion being very painful.

Upon this case I applied my modification of Wutzer's instrument, using a plug 3 inches in circumference, big enough to fill the ring well—length of wooden plug invaginated $2\frac{1}{2}$ inches, distance of exit of needle from edge of *doigt de gant* $3\frac{1}{4}$ inches. The surface of the wooden plug was painted over by a very elegant preparation of cantharidine held in a solution of collodion; one grain of the former to one ounce of the latter. The instrument was applied for nine days, and on its removal the usual treatment of spica bandage and compress was applied.

The case has been submitted to the examination of very many of my professional friends, and it is found to be most radically cured. I intend operating on the other side shortly.

These complete the list of femoral hernia.

James P—, æt. 26, has been the subject of a ventral hernia for four years; the result of a strain. In size it is about that of a pigeon's egg.

The plan here adopted was by wires, and executed in the following way:

The hernia having been returned, the curved trochar containing the needle before alluded to was carried by the finger well through the ring, and its extremity made to press against the abdominal wall. The needle thrust through them, and, having been threaded by a silver wire, withdrawn. The same manœuvre on the opposite side of the ring (or in more places according to circumstances). To the lower ends of the wires were attached small pieces of the india-rubber and secured by split shots, and to the upper similar but larger pieces. They were then drawn tightly together and clamped by split shots. A portion of integument was thus tucked into the ring.

At the end of eight days the wires were removed. A copious and thick purulent discharge issuing in their track, and considerable thickening of the

surrounding tissues. The portions of the wire contained in the body were quite bright.

In about one week he was allowed to get up, and a light truss applied, the hernia being cured; impulse on coughing the same on both sides of abdomen, and no evidence to the finger of the former aperture.

W. P—, æt. 55, the subject of ventral hernia. The chief point of remark in this case is that he was operated upon twenty years ago for a strangulated scrotal hernia, and which would seem to be radically cured by a portion of omentum blocking up the external ring.

Since this operation he has been the subject of a ventral hernia which was operated upon by similar means to the latter case, and with a similar result.

2. Mr. T. Spencer Wells' Case.

V. F—, æt. 29, cook at one of the clubs, came to me in August last with an oblique inguinal hernia on the left side. The canal was very short, and the ring just admitted my index finger. I performed Wutzer's operation on August 13th, removing the instrument on the 20th. He had not suffered in any way. The puncture was healed on the 28th, when he put on a belt truss. This he wore for two months, when the canal appearing to be firmly closed, I permitted him to leave it off. He went on well until last month, when he thought there was some bulging at the external ring, and on examination I found that it was not completely closed. He was very anxious to have his cure perfect, and as there was not room to pass up even a small cylinder, I determined to try the effect of a metallic seton. Accordingly on January 3d, I pushed up a fold of scrotum to the ring, and passed a handled needle along my finger beneath the tendinous border of the ring, and along the canal for about an inch, then bringing it out through the parietes. I then passed one end of an iron wire through the eye in the projecting point of the needle, and by withdrawing it of course drew the wire—or seton—through the canal. I fastened each end over a wick of stocking cotton, and left the patient quiet in bed. No inflammatory action whatever was set up for three days, and I told him to get up and walk about, the seton being still in its place. This brought on a little swelling, but he went about for four days longer, when I removed the wire, as it appeared to be surrounded by hard exudation, and some sero-purulent discharge exuded from the points of puncture. He then reapplied his truss, and the ring when I saw him last week appeared to be very firmly closed.

ART. 108.—*On the treatment of Hernia by Electricity.* By Dr. CLEMENS.

(*Deutsche Klinik*, 39, 1858; and *Med.-Chir. Review*, Jan., 1859.)

This paper is the first of a series the author intends publishing upon the therapeutical application of electricity—a subject that has engaged his attention for some years past. He first employed this agent in the treatment of inguinal hernia, in 1850, and has frequently had recourse to it since then. The hernia being reduced, and the patient placed in the semi-recumbent position, the ball of the conductor is carried as far into the hernial canal as possible, and the application of the electricity continued during five minutes, its power being increased day by day. After a few *séances* the mouth of the ring becomes diminished in size, the finger is introduced with more difficulty, and the hernia will not descend so easily as heretofore. The electricity, too, exerts a very beneficial

effect upon the peristaltic intestinal motions, augmenting and regulating these, and thus preventing the same relaxed portion of intestine from always lying opposite the hernial aperture. A state of obstinate constipation becomes changed for one of regular action, and many old disordered conditions of the abdominal cavity become relieved. When the hernia has been recently produced, no means act with so much certainty and rapidity; and a case is referred to of a young man who acquired double inguinal hernia during an effort to raise a heavy burden, and which was completely cured after twenty *séances*, although these were not commenced until a week after the accident. Under its agency recent hernia is rapidly returned; but the author has not yet tried it in a case of complete incarceration. Among the twenty-seven cases in which it has been resorted to, none have manifested the slightest ill consequences. Dr. Clemens prefers static electricity to galvanism, and administers it by means of the Leyden phial.

Another application of electricity by the author consists in a galvanic hernia truss, for a description of the construction of which we must refer to his paper. By its agency a feeble but constant galvanic stream is kept applied to the ring, and large hernias soon become easily retained which before had resisted the largest trusses and the strongest springs. Of late, the author has constructed a pile of silver and copper coins, and the effects of so small an apparatus have often surprised him.

ART. 109.—*Lithotomy simplified.*

By Dr. R. T. CORBETT, Surgeon to the Glasgow Royal Infirmary.

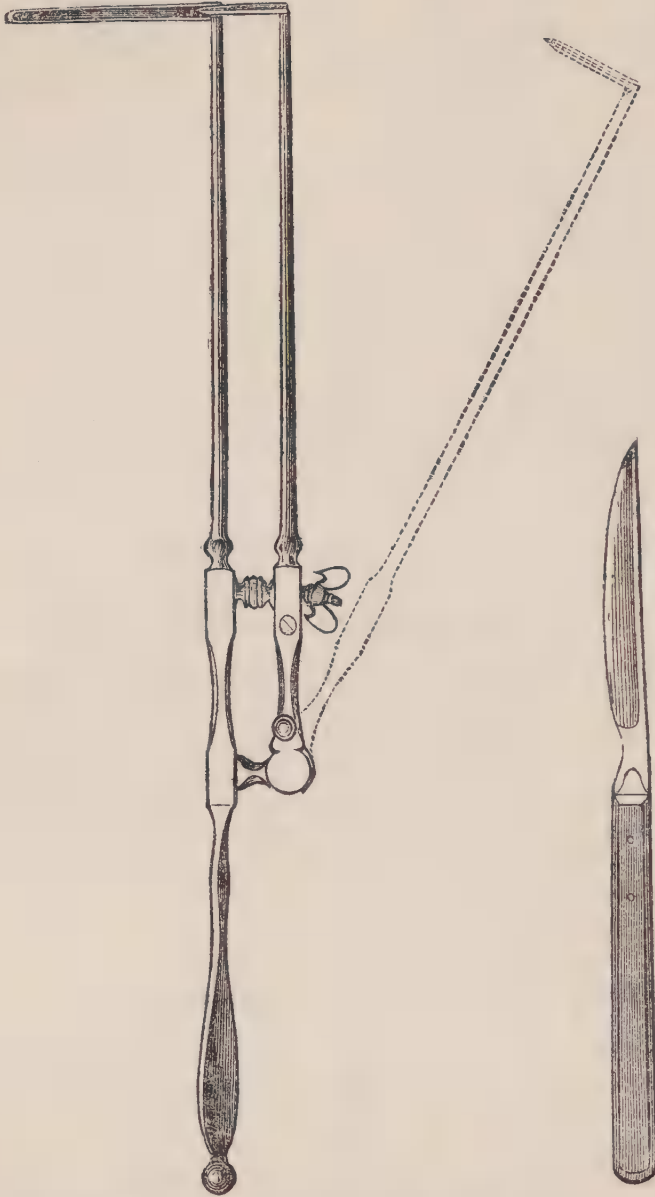
(*Medical Times and Gazette*, Dec. 11, 1858.)

Dr. Corbett's instrument is composed of two staffs. The inner staff is for introduction into the bladder, and it is furnished with a handle, by which it is kept steady when wished; the other is for application outside, and is attached to the first by a catch joint, which is fixed to a projection under and behind the handle, and is steadied by a quadrant-looking projection which passes through it. As this projection has a screw upon its upper and under surfaces it permits the action of a nut-screw.

When the instrument is secured *in situ* for the operation, the perpendicular portions of the staffs are parallel to each other; and the short horizontal limb of the outer, which is pointed like a pen and grooved upon the left side, enters a similar groove upon the same side of the horizontal part of the inner staff, by means of which a direct groove or tract is opened into the bladder. The staffs of the instrument can be detached and united with the greatest ease and quickness.

"To make myself fully understood," says Dr. Corbett, "I shall suppose that a patient under the influence of chloroform has been placed upon the table for operation. I first introduce that part of the instrument which is the larger of the two pieces of which it is composed, and which is in reality a rectangular staff; having struck the instrument against the stone to be satisfied that there is one, I then get the patient's feet bound to the thighs—which is much safer than binding them to the hands, as this latter practice draws the patient's body too much together, and materially lessens his capacity for breathing, thereby increasing the danger

when chloroform is used. The nates now being brought to the edge of the table, the upper part of the pelvis elevated by a pillow placed below it, and the knees well separated, I take the handle of the staff in my right hand, introduce the forefinger of my left into the rectum, and feel for the horizontal part of the instrument; being satisfied that I feel the prostate between my finger and the portion of the staff just named: I



then move the instrument slightly backward and forward until the angle rests immediately in front of the anus, of which position I am assured by my left thumb resting upon it. The handle at this moment being perpendicular, I now desire my assistant to hold it steady, and I attach the outer portion of the instrument, which is effected in a moment by means

of the spring-catch. The quadrant-looking projection from the first staff passes through the outer one ; and this contrivance, with the joint named, being what is called a double-joint, completely does away with lateral motion. The outer portion is then pressed down with the right hand till its point touches the skin immediately in front of the anus, and in the middle line. The nut is now fitted on to the quadrant projection, when a single turn of the screw is sufficient to establish a line of communication with the bladder. The knife used with this instrument being held at a right angle to the outer staff, is then run along the groove into the bladder till its point is stopped, and a curved incision of an inch and quarter, or inch and half in length, is made to the left of the rectum, as the knife is being withdrawn. The operator then introduces the fore finger of his left hand into the bladder, and dilates the opening in the prostate if he thinks fit. The staffs having been withdrawn, the stone is removed by any instrument the operator may deem most suitable for the purpose.

"The knife I use is exactly three eighths of an inch broad, perfectly straight in the back, which is ground to a blunt edge till half an inch from its point, where it is cutting, being fitted to pierce as well as cut. The blade from its point to its shoulder is equal to the length of the two grooves when the instrument is screwed up ; this shows how far the knife should enter ; but it is not essential that the blade should be of any particular length, as the knife is thrust home until its point is stopped by the closed extremity of the groove. In front of the shoulder of the blade there is a depression which is rounded off, in which the fore finger rests while the knife is being used. I would recommend the finger in the rectum to be applied to the horizontal portion of the staff while the knife is being thrust into the bladder, as it is unnecessary to press the rectum away from the edges of the knife, this gut never having been known to be cut by this method of operating.

"The points in which I think this instrument possesses advantages over Dr. Buchanan's are these :

"1st. That you transfix the very commencement of the prostate, where the membranous portion of the urethra ends, by which you render it impossible for the angle of the staff to shift from the position in which it has been placed, thereby rendering yourself independent afterwards of your assistant ; and it follows from the instrument being fixed in the place of election, that you can with far greater certainty cut the gland in the place wanted, and with the greatest exactness to any extent desired.

"2d. A deep groove leading from the outside into the bladder, that organ can be cut into with the greatest confidence, ease, and certainty, whereas with the rectangular staff, in fat subjects, or when from any cause the perinæum is deep, there is sometimes great difficulty in finding the groove, and even in young subjects I have seen much time lost, and worse than that, before the knife was fairly introduced.

"In using the rectangular staff, when an operator gets his knife entered at once into the groove, I am almost disposed to think, that either chance or good luck has had something to do with the matter ; and if I should ever operate again by this staff in an adult, I will most certainly make a preliminary incision round the left side of the anus, find

the angle of the staff, and then enter the knife in the groove; but I trust the instrument I have now attempted to explain will obviate this necessity.

"I have operated frequently upon the dead body by means of this instrument, but have used it only once upon the living; this was three days ago, on the 9th of the current month, at 9 o'clock a.m., in the operating theatre of the Glasgow Royal Infirmary, in presence of many practitioners and students. The subject of this operation was a lad, æt. 17, who had been afflicted with symptoms of calculus from his infancy. The sufferings he experienced from that early period had affected his general health to a considerable extent, and he looked like a boy of 14. Slight exertion produced great pain, and there was considerable mucous deposit from his urine.

"The operation was performed with the greatest ease, so far as the instrument and the use of the knife were concerned, and a stone measuring $2\frac{1}{8}$ inches in length by $1\frac{3}{4}$ in breadth, and weighing two ounces three drachms and a half, was extracted by means of the forceps, which caught it at once. As the calculus was, however, very large, and the forceps used relatively small, some exertion was necessary before it could be brought through the opening in the bladder, although the prostate was undoubtedly cut and dilated previously as far as seemed safe. The boy lost little blood from the operation, the urine afterwards came freely away by the wound, the edges of which continued soft and free from all inflammatory swelling. He suffered as little pain as is generally experienced after this operation, but his pulse was weak, and he could not be prevailed upon to take nourishment; and he continued in this state till his death, which took place forty-four hours after the operation, the patient having had two healthy alvine evacuations about two hours before his decease. Permission to inspect the body was not granted, although urgently requested; but a young gentleman who was officiating *pro tempore* as my clerk, was allowed to make a small cut into the hypogastric region, when he saw that the prostate had been cut as nicely as possible, but the bladder was very much thickened, the result, no doubt, of chronic inflammatory action; there was no ecchymosis of its mucous surface, but the serous covering was reddened, and there was some effusion into the peritoneal cavity; but as a thorough inspection was not made, it remains doubtful what was the immediate cause of death. If I ventured upon an opinion, I would say, that subacute peritonitis, proceeding from a diseased bladder, was the cause. It may be remarked by some, that the principle of this operation is nothing new, in other words, that means of establishing a route for cutting-instruments into the bladder have been tried by several surgeons in times past—this I have found out, since Mr. W. B. Hilliard, of Glasgow, made this instrument for me, now two years ago; but at the present moment I am only acquainted with two instruments so constructed, viz., those which bear the names of Sir James Earl and M. Guerin."

ART. 110.—*Statistical analysis of 186 Lithotomy operations.*

By Mr. JONATHAN HUTCHINSON.

(Medical Times and Gazette, Jan. 8, 1859.)

In the subjoined analysis, all the cases of lithotomy are included which occurred between January, 1854, and July, 1857, at the following Hospitals, viz.:

University College, King's College, St. Bartholomew's, St. George's, Guy's, St. Thomas's, the London, the Middlesex, the Westminster, Charing-cross, St. Mary's, the Metropolitan Free, the Marylebone, the Hospital for Sick Children, and the "Dreadnought" Seamen's Hospital.

During the three years and a-half over which these statistical reports extended, 186 cases of lithotomy are recorded as occurring in the different metropolitan hospitals. It thus appears that an average of 40 patients a-year are operated on for stone in the bladder, or in other terms, not quite one a week. This number, seeing that the list comprises thirteen hospitals, several of them large ones, is certainly smaller than might have been expected. The modern practice of crushing has no doubt rendered this number somewhat smaller than it would otherwise have been; but that it has not greatly diminished it, we shall have to show in a subsequent analysis of the lithotripsy operations performed during the same period.

Of the 186 cases, 146 resulted in recovery, and 40 ended in death. Of the whole number, 137 were under the age of 20, and of these 123 recovered and only 14 died; while of the 49 in which the patients were adults, we find but 23 recovered, and no fewer than 26 died. These figures show in a very strong light the influence of the age of the patient upon the prospects of a lithotomy operation. We shall now proceed to exhibit the extent of this influence in greater detail, and then to examine as to the several causes of the fatality of this operation, and their relative importance.

Influence of the Patient's Age on the prospect of Recovery.

In the subjoined table the cases have been classified according to the patient's age—

Age.	No.	Recovered.	Died.	Per Cent. of Deaths.			
1	1	1	0	0	1 in 8	Under 10 years of age. { 8 deaths in 109 cases, or 1 in every 13.6.	
2	8	7	1	12.5			
3	24	20	4	16.6			
4	15	15	0	0			
5 to 8	43	40	3	7	1 in 14		
8 to 10	18	18	0	0	1 in 3.5	Between 10 and 25 years of age. { 7 deaths in 36, or 19.4 per cent. or 1 in every 5.	
10 to 15	21	15	6	28.5			
15 to 20	7	7	0	0			
20 to 25	8	7	1	12.5	1 in 8		
25 to 30	3	2	1	33.3	1 in 3		Between 25 and 45. { 4 deaths in 8, or 50 per cent. or 1 in every 2.
30 to 35	3	1	2	66.6	2 in 3		
35 to 40	2	1	1	50.0	1 in 2		
40 to 45	0	0	0	0	2 in 3	Between 45 and 60. { 9 deaths in 17, or 52.8 per cent. or more than half.	
45 to 50	3	1	2	66.6			
50 to 55	5	3	2	40.			1 in 2.5
55 to 60	9	4	5	55.5	1 in 1.8		Between 60 and 75. { 12 deaths in 16, or 75 per cent. or 3 in every 4.
60 to 65	7	1	6	85.6	6 in 7		
65 to 70	7	3	4	57.0	4 in 7		
70 to 75	1	0	1	100.			
75 to 80	1	0	1	100.			
Total ...	186	146	40				

Thus it would appear that between the ages of 8 and 10 is the period in which the lowest death-rate after lithotomy prevails. Of 18 operations performed on patients between these ages all resulted in recovery. If we group together all between 5 and 10 we shall have 61 cases, out of which only 1 in 20 ended fatally. Children under the age of 5 appear to bear the operation not so well as those a little older, since of 48 operations 1 case in 9 ended in death. If we pass by the fact that no death appears to have occurred out of the 7 cases between the ages of 15 and 20 as probably (on account of the smallness of the number) a coincidence, we may allege that amongst adults the rate of mortality rises with the age of the patient. Of those under 10 only 1 death in 13.6 cases occurred; in those between 10 and 25 the rate has risen to 1 in 5, in those between 25 and 45 it has reached 1 in 2; between 45 and 60, it is actually more than half; while subsequent to the age of 60 it attains the frightful proportion of 3 in every 4. There can be no doubt that this appalling mortality in patients of advanced age is in part produced by the fact that of late years the best subjects have been treated by lithotripsy, and that, in the hands of many surgeons, only those patients not considered to be in sufficiently good health to bear the latter have been submitted to lithotomy. Still we regret to know that this circumstance has but a very limited application, since lithotripsy has been practised to a very small extent.

The explanation of the comparative freedom from risk in young pa-

tients, is to be found in the fact that disease of the kidneys is a very common concomitant of vesical calculus in grown-up persons, and a very rare one in children. The tables about to be adduced, exhibiting the cause of death at the several ages, will show that of all the various evil influences, renal disease is by far the most important. In the list of 14, however, in which death was referable to it, we find only 3 cases in which the patients were under 20, and only 1 in which the child was under 10. On the contrary, of the 4 who died of sthenic peritonitis, all were under 90, and 2 under 10. Peritonitis, hæmorrhage, and shock of the operation, rank as the chief causes of death in childhood.

Much importance has been attached by some writers to the weight of the stone as a means by which to estimate the risk incurred. The heavier the stone, the greater the danger. To Mr. Crosse, of Norwich, we owe a valuable collection of data on this point. Quoting Mr. Crosse's tables, Mr. Coulson, in his work on diseases of the bladder, actually goes so far as to assert—"The chance which a patient has for recovery can, therefore, be calculated beforehand, and, independent of every other consideration, from the ascertained dimensions and weight of the stone." The statistical fallacy here involved is transparent. Of course children have, as a general rule, much smaller calculi than adults; and, as we have seen above, they are by very far the best subjects for lithotomy. The cause of the freedom from risk in children is not, however, to be found in the fact that their calculi are lighter, which is little more than a coincidence, but in their usual freedom from renal disease. Of course no one will deny that the large size of a stone is a circumstance of some prejudice to a patient's prospects. A surgeon would however, commit an absurd error if he should imagine that a boy of ten, from whom he had removed a mulberry calculus weighing several ounces, had only an eighth part of the chance of recovery which was possessed by a man of sixty from whom a phosphatic one of not as many drachms in weight had been removed. In all probability the reverse estimate would be nearer the truth. The fallacy to which we have referred, viz., that difference in weight of the stone is, as a rule, coincident with difference in age of patient, appears to us to make any statistical calculations of weights quite valueless for purposes of prognosis. All that can be said is that of patients of equal age and similar degree of health, those who have large calculi have a somewhat worse chance of recovery than the others, and even this rule must be allowed to receive constant modifications, from consideration of the composition of the stone in each individual case. Leaving what we have to adduce, as to the influence of the previous health of the patient upon his chances of recovery after lithotomy, to another part of the report, we will now pass to—

Causes of death after lithotomy.—In the following list we have endeavoured to classify the cases according to the accident or lesion which appeared to have exerted the most efficient influence in bringing about the fatal event. In many cases, of course, more than one had been at work at the same time. Thus some cases in which the main lesion appeared to be disease of the kidneys had also inflammation of the bladder and of the pelvic cellular tissue; while in others, which died ultimately of some distinct visceral disease, hæmorrhage occurring during or after the operation, might be reasonably supposed to have diminished the

patient's chance and predisposed him to the attack. The classification is therefore only an approximation to the truth, and must be so considered. Proceeding on this plan, we find that of the forty cases ending fatally, death was referred to

Renal disease	in 14 instances.
Hæmorrhage	4 "
Pyæmia	4 "
Peritonitis	4 "
Shock of operation	2 "
Extravasation of urine	2 "
Abscesses about the bladder	2 "
Wound of the fundus of the bladder by the knife	2 "
Exhaustion	1 "
Convulsions	1 "
Cystitis	1 "
Bronchitis	1 "

In the remaining two cases we do not possess sufficient detail to allow of our assigning them to any class without risk of error.

ART. 111.—*New method of curing Hydrocele.* By Professor SIMPSON.

(*Edinburgh Medical Journal*, Dec., 1858.)

Many methods have been proposed of treating hydrocele surgically; and at the present time, tapping, and medicated injections into the sac, form the means generally resorted to. The new method which Dr. Simpson suggests, is founded upon the fact that iron and other metallic wires, when placed in contact with living tissues, do not, as a general law, excite inflammation to a higher stage than that of adhesion, or the effusion of coagulable lymph. Dr. Rothmund, of Munich, performed the radical cure of hernia by exciting adhesive inflammation in the returned hernial sac, passing, for this purpose, and leaving for eight days, a metallic needle traversing the peritoneum; and he has not, it is averred, lost a single patient out of 1000 operated on. If metals in serous sacs created a higher stage of inflammation than the adhesive, such a fortunate result as this would not have been attained. Dr. Simpson thinks that metallic wires passed through the sac of a hydrocele would act in two ways: first, they would drain off the fluid; and, secondly, they would subsequently, by their presence, form the surest means of exciting the subsequent amount of adhesive inflammation that was required for the cure of the disease. Dr. Young has, in one of his patients, afforded him an opportunity of putting this idea to the test. The slender wire or metallic seton which was used in this case was passed through the sac by first traversing it from below upwards with a long-handled surgical needle, such as is used in transfixing and tying hæmorrhoids, threading the eye of the needle, after it was projected through the scrotum above, with three or four slender iron threads, pulling the needle then backwards through the sac and out, and thus leaving the metallic seton in its place. The liquid drained off in an hour or two;

adhesive inflammation set in, and progressed for two days, when it began to subside. The wires were removed on the third day; and the cure has remained apparently quite complete, with the vaginal sac firm and consolidated. Dr. Young has published the case at length (*vide* next article). This method of treating hydrocele is, Dr. Simpson holds, much simpler than tapping and injecting; not by any means so painful to the patients; less likely to produce a suppurative or dangerous amount of inflammation; and, perhaps, experience will show that it is surer and more certain in its results.

ART. 112.—*On the treatment of Hydrocele by the iron-wire seton.*

By Dr. JAMES YOUNG.

(*Medical Times and Gazette*, Feb. 26, 1859.)

The treatment adopted in the two following cases is that referred to in the previous article. It is evidently one of great promise.

CASE I.—About the middle or end of June last, J. L—, æt. 40, received while in bed a kick upon the testes, from one of his children who was sleeping with him. The blow occasioned no pain at the time, but when he rose in the morning, he felt it somewhat painful, and the left testicle swollen, and during the succeeding eight weeks it continued to enlarge, without being accompanied with any great degree of pain.

About the end of August the patient describes the scrotum as getting "soft and doughy," and increasing gradually in size, until the beginning of October, it measured five or six inches in depth.

On October 1st, the patient placed himself under my care, when, after examination, I recommended an operation, which was eventually performed on the 26th of that month by the insertion of an iron-wire seton.

This new treatment was adopted in consequence of Professor Simpson having become acquainted with the case.

We resolved to give this new method of curing hydrocele a fair trial, and accordingly the operation was performed by passing a large curved needle through the scrotum and tunica vaginalis, for about two inches; five fine iron wires were passed carefully through the eye of the needle, and then it was withdrawn, leaving ten threads as a metallic seton, which were allowed to remain, as in the case of a common seton. The fluid began to flow immediately, and continued to ooze out for some time after, soaking several towels, until the whole escaped. The patient complained of very little pain either during the operation or after it.

On October 27th the inflammation began (a little fluid continued to trickle), and on the 28th the swelling was considerable.

As the action was sufficiently powerful on the 29th, the seton was removed with little pain to the patient. We observed, on removing the wire, a considerable quantity of lymph on it, the result of the inflammation. The swelling began to fall every day after the removal of the seton, and the patient rose on November 1st.

At the present date, February 18th, 1859, I may state that he continues quite well, without the least return of the hydrocele, the scrotum having regained its usual healthy size.

This, so far as I am aware, is the first operation of the kind that has been performed in Edinburgh, and reflects the greatest credit on Professor Simpson, who proposed the operation. The complete success which attended

this case, encouraged me to make further inquiries, and I have since operated on two other patients with perfect success; but I will merely mention one more here, as the operation is conducted on the same principles in every case.

CASE 2.—R. W—, æt. 17, by trade a blacksmith, states that, about two years ago, he observed his testicle begin to swell, and continue to enlarge from below upwards.

In August, 1858, the scrotum was about the size of two large turkey eggs. At that time he had been treated with blisters, &c., but without any permanent benefit; and at the beginning of this year he was advised to come to Edinburgh, and undergo some operation to obtain relief. I saw him for the first time on January 5th, 1859, and proposed this new operation to him.

In this case the patient was not aware of having received any external injury.

On January 6th the operation was performed, along with Professor Simpson, in a similar way as described in the previous case, except that only four wires were used instead of five as before.

The fluid escaped rapidly, and in large quantity. The patient complained of very little pain, either during the operation, or the first twenty-four hours. On January 7th the scrotum was much reduced in size, but on the 8th the inflammatory action commenced, and on the 9th the scrotum was considerably larger, and as the patient was complaining of great pain, we thought it advisable to remove the seton. On the 10th and 11th there was a slight discharge of pus from the upper wound, which was treated by the application of a poultice, and the lead and opium lotion. On January 12th the swelling began to fall, and the patient got rapidly well, and was soon able to go home.

ART. 113.—*Case of Malignant Disease affecting a testicle which had been retained within the abdominal cavity.* By Dr. GEORGE JOHNSON, Physician to King's College Hospital.

(*Proc. of the R. Med. and Chir. Soc.*, Jan. 11, 1859.)

CASE.—The subject of this history, C. D—, aged twenty-seven, was a well-developed, muscular man, of active habits, residing at Cambridge as a private tutor. The disease which ultimately caused death, appears to have commenced about the month of September, 1857. At that time, while out shooting, an uneasiness low down on the right side of the belly, which had been felt for a short time before, grew into such intolerable agony that he had "to knock up" for two hours. The pain then went off, and he finished the day's sport. From that time the pain was more or less constant and severe, and on several occasions greatly aggravated by active exercise. Dr. Johnson was first consulted by letter in April, 1858. The description of the pain, and its situation in the course of the right ureter, suggested the notion that a calculus might be impacted in the ureter. Some questions relating to the effect of the pain upon the testicle, elicited the fact that the right testicle had not descended from the abdomen. A careful examination on the 17th of April failed to discover any tumour in the right inguinal or iliac region. The patient's general health and nutrition were at this time but little affected. The urine was of high density, and deposited lithates and oxalates, but contained no other abnormal products. The pain continued, and the patient began to lose flesh and strength. It now occurred to Dr. Johnson, as a pro-

bable explanation of the symptoms, that the retained right testicle had become the seat of malignant disease. The patient's mother had died of cancer of the stomach. On the 12th of June there was a consultation with Dr. Bright. At that time there was decided evidence of a tumour or deposit in the abdomen, above Poupart's ligament, on the right side. The tumour rapidly increased, until it extended much beyond the median line of the abdomen, above the umbilicus, and even to the epigastrium. The patient meanwhile became much emaciated, and died exhausted on the 7th of July. On post-mortem examination, the right testicle, situated in the abdomen, was found to be the seat of medullary disease, forming a tumour weighing sixteen pounds, even after the escape of about four pints of grumous fluid from some large cysts which had become developed in it. The lymphatic glands in the abdomen were free from disease.

ART. 114.—*On injections in Gonorrhœa.* By Professor SIGMUND.

(*Schmidt's Jahrbuch*, Bd. xcvi, p. 49; and *Medical Times and Gazette*, Nov. 6, 1858.)

Professor Sigmund, of Vienna, as the result of his extensive observation in this class of diseases, is decidedly in favour of the employment of injections in the treatment of gonorrhœa. He believes that those who have derived no benefit from their use, or who have observed mischievous consequences from this, have, in the great majority of cases, employed them improperly. He has tried injections with balsam of copaiba, and with chloroform, but has given them up as unpractical, and those made with the patient's own urine, while taking balsam copaiba, were found to be as inert as water. From among a large number of substances tried, he confines himself now almost entirely to sulphate and acetate of zinc or lead, alum, and tannin; and of these he prefers the sulphate of zinc to all others, because the great majority of patients are cured by it; it acts mildly, neither soiling the linen nor changing the colour of the urine, and it is very cheap.

For injections to succeed, they must be used at the proper time, in a suitable dose and manner, and they must be continued sufficiently long. The period for their employment has arrived as soon as the inflammation of the mucous membrane of the urethra has become subdued; but they should not be used as long as there is present considerable swelling, great, or even slight, if continuous, pain, spasms, or frequent calls to pass urine. The dose of the material should be small, as five grains to the ounce of extract of lead, one quarter of a grain of nitrate of silver, one grain of sulphate or acetate of zinc, &c. It is seldom necessary to increase the original dose. The addition of anodynes, as opium, hyoscyamus, &c., has no advantageous effect. We should carefully teach the patient how to use the injection; and a small tin syringe, with a conical tube, is to be preferred. It should hold at least two drachms. The patient should be placed in the upright position, and should pass urine prior to the injection being thrown in. The tube must be so passed into the urethra, that no fluid can flow out between the canal and the tube. The fluid is now to be slowly thrown in, and then the mouth of the urethra is to be kept closed by two fingers, so that nothing can pass out during two or three minutes. Two injections are to be thrown

in one after the other, and they are to be repeated three or four times daily. The injections should not be thrown in just before going to sleep, as they then sometimes give rise to seminal discharges. They must be persevered in for eight or ten days, after all traces of diseased secretion have ceased to be visible, even in the morning. The average time required will be from twenty-one to twenty-eight days. Internal means may also, if desired, be employed, and balsamic medicines in many cases hasten the cure.

Dr. Sigmund rarely has recourse to caustic injections, as the nitrate of silver, sulphate of copper, chloride of zinc, &c., because generally the experiment is dangerous. He limits their use to simple, uncomplicated gleet, which has resisted the usual means, as also to recent gonorrhœa without inflammation occurring to persons who have already employed the treatment with advantage.

(C) CONCERNING THE INFERIOR EXTREMITIES.

ART. 115.—*On Compression of the Artery in inflammation of the limbs.*
By Professor VANZETTI, of Padua.

(*L'Union Méd.*, Nos. 58 and 115, 1858; and *Medical Times and Gazette*, March 19, 1858.)

“Professor Vanzetti has resorted to manual compression in the case of inflammation of the limbs, whenever the artery has been accessible; and such success has attended its application to the femoral, brachial, and subclavian arteries in phlegmon and arthritis, that it now forms the ordinary means of treating such cases at the Padua Clinic whenever it is practicable. Employed promptly it soon may arrest the progress of the inflammation, more time being required when the process has been set up during some period. In the case of severe or much advanced inflammation, although not of itself competent to effect a cure, it is the most powerful adjuvatory means. In the face of the great advantages derivable from this means we must not be deterred by apparent difficulties in the execution. Persons can be generally got to make the compression, or the patient may be taught to do this; and, in a case of great urgency, the surgeon himself should perform it for two or three hours. In general, compression need only be kept up for eight or ten minutes, and, after resting, again resumed. The Professor relates two cases as remarkable examples of the efficacy of this treatment: one being an instance of a bad phlegmonous erysipelas of the arm, treated by compression of the subclavian; and the other a case of acute arthritis of the wrist, treated by compression of the brachial.”

ART. 116.—*Excision of the entire fibula.* By Dr. A. REEVES JACKSON.

(*Amer. Jour. of Med. Sciences*, Oct., 1858.)

CASE.—Mrs. R., æt. 37, the wife of a farmer, and the mother of four children, noticed some time during the month of May, 1849, a painless swelling of the outer side of the calf of the right leg, about four or five inches

below the knee-joint. The enlargement slowly increased, and extended itself upwards and downwards towards the knee and ankle-joints. The right foot and lower part of the leg became œdematous, and she walked with a slight limp which she attributed to weakness of the part. Her general health continued unimpaired. She attended to her ordinary household duties until the early part of September, when in stepping down stairs, she felt "something give way," and fell to the bottom. She was now unable to walk, and for the first time applied for medical assistance.

Dr. G. A. Kaski, of Bartonsville, Pa., was sent for, who, after attending the case a few days, and ascertaining its unusual character, requested my attendance in consultation.

Accordingly, we saw her together on the 19th September. On examination we found the leg much swollen, painful on pressure, and the foot considerably everted. There seemed to be evident fracture of the fibula, although not the least crepitus could be produced. The leg was lightly dressed with splints and bandages, and cooling applications ordered to be applied.

I saw nothing more of the case for nearly three months, when I was again requested by Dr. Kaski to see her.

At this time (Dec. 10th) the leg was much more enlarged, and the patient complained more of pain. The splints and bandages had been dispensed with for several weeks, owing to the irritation of the integuments which they produced. The eversion of the foot was still more marked than before. The general condition of the patient was indeed lamentable. She was greatly emaciated, although she did not present the appearances of a person suffering from malignant disease. Extensive ulcerations had occurred over the hips and sacrum, resulting from her long confinement to bed, and the discharges from which had seriously drained her system. There was much œdema of the parts about the ankle, foot, and on the fibular aspect of the leg. No fluctuation could be detected at any point.

Her condition was now such as to render it clear that something must soon be done for her relief, or that she would sink from exhaustion. Amputation of the limb had already been suggested to her, but to this measure the patient as well as her friends firmly objected. Excision of the affected bone was now proposed, and the nature of the operation having been explained to her, she consented to have it done. It was accordingly decided to excise the bone on the following Monday. In the mean time, she was put upon a course of tonic treatment, and the use of the most nourishing diet enjoined upon her.

Operation.—The operation was performed December 22, 1849, in the presence and with the assistance of Drs. Kaski and Foss, and Messrs. Kübler and Shick.

The patient having been put fully under the influence of chloroform, the leg was partly flexed, and a longitudinal incision made, commencing about a half inch above, and an inch in front of the head of the fibula, and extended downward to the external malleolus, dividing the skin and peronei muscles. A second incision, starting from the same point as the first, was made transversely, and carried directly backwards about two inches. The flap thus formed was rapidly dissected off from the bone (or rather what was formerly bone) until the upper four inches were fully exposed. I now made an attempt to detach the head of the bone from its tibial articulation, but found it a very difficult proceeding. However, the substance representing the fibula, flexible and much thickened, was finally separated by passing a narrow-bladed bistoury between it and the tibia, the edges of the wound being held widely apart, at the same time, by an assistant, with blunt hooks. Great care was

necessary at this stage of the proceedings in order to avoid wounding the anterior tibial nerve, which was here seen passing down.

The upper portion, being now detached, was drawn outwards and made use of as a lever to aid in separating the remainder. Seizing it with the left hand for this purpose, the fibres of the peroneus longus and the interosseous ligament were divided, the knife being kept close to the bone. In this manner, it was also separated from its connexions with the soleus and the flexor longus pollicis pedis. Proceeding downwards, I found the attachments of the muscles and the interosseous ligament to the bone were so slight about its middle third, that they were readily separated by the handle of the knife. At one point there was scarcely any vestige of bone remaining.

The most difficult part of the operation consisted in removing the lower end of the bone from its attachments to the fasciculi of the external lateral ligament of the ankle-joint and the several short ligamentous attachments to the tibia. It was finally accomplished, however, by making a third incision, perpendicular to the first, at its lower end, about an inch and a half long, and dissecting the flaps carefully back; then by pulling the diseased mass strongly outward, sufficient space was obtained for dividing the connexions with a slender knife. Care was taken to avoid injuring the small slip of synovial membrane which is here sent up between the tibia and fibula from the ankle-joint.

The tibia and astragalus were both found to be unaffected by disease.

There was very little arterial hæmorrhage throughout the operation. The peroneal artery, which was the only one of great size that was really in danger, was carefully avoided. Some of the anastomosing branches of the anterior and posterior peroneal arteries were cut, but torsion being applied to them, the bleeding soon ceased. The venous and capillary bleeding was troublesome for a time, but finally ceased under the application of pressure and the use of cold water.

Dressing and progress of cure.—The edges of the wound were brought together and retained by several points of interrupted suture and adhesive straps, the whole being covered lightly with a roller bandage. A well-padded splint, four inches wide, and extending for a short distance above the knee-joint to four or five inches below the foot, was placed along the tibial aspect of the limb, and confined by a few turns of the roller above the knee and to the foot, in such a manner as to draw the sole of the foot more nearly to its natural position. Cold-water dressing was then ordered, to be kept constantly applied to the wound. A full dose of Morphia was given at bedtime.

The sutures were removed on the third day, at which time union was found to have occurred throughout the greater part of the wound. The patient was kept upon the use of tonics, and a full diet allowed. The subsequent progress of the case was entirely satisfactory; and at the end of three weeks the patient was able to sit upon a chair with the foot and leg resting upon an elevated cushion. In two and a half months, she was able to walk with a cane, by the aid of a stout gaiter-shoe, to the sole of which was attached a steel plate three fourths of an inch wide and one eighth of an inch thick, which, being applied to the outer side of the limb, extended upwards to a point opposite the tibio-fibular articulation, and the upper end, being well padded, was secured by a broad strip of ferreting passed around the leg below the knee. This apparatus she used for about two years, when she at length threw it aside, and ever since has merely used a cane.

Functions of the leg and foot.—She walks with a slight limp, bearing the weight of the body upon the inner side of the foot, the sole of which is con-

siderably everted. Owing to the detachment of the biceps muscle, she has no power to evert the leg when in a semi-flexed position. The soleus, however, continues to act from its tibial attachment. There is a tendency in the foot to be partially flexed from the detachment of the peroneus longus and peroneus brevis, those muscles being extensors of the foot, and antagonistic to the tibialis anticus and peroneus tertius, which flex the foot. The motion at the ankle-joint is somewhat impaired, owing, perhaps, to the long-continued eversion of the foot and the consequent side-pressure on the joint.

Appearance and pathological condition of the bone.—It is thickened throughout its entire extent, but most so about the middle and upper extremity. At the largest point it measures five and three quarter inches in circumference. It is of yellowish-white colour, in consistence rather softer than cartilage, and thickly studded with osseous spiculæ imbedded in a dense, elastic, fibrous tissue. Its external surface is rough and irregular, and presents numerous small cavities of varying size. The periosteum is entirely removed from the posterior surface of the upper half of the bone, and, although still present on some parts of the anterior surface, it is much thickened and softened. At one point there is no bone whatever for a distance of an inch and a half, the upper and lower extremities being held together merely by a few shreds of periosteum. The lower part of the bone is tolerably firm, enlarged to about twice its normal size, and the periosteal investment unaltered. The interior of the bone, however, is softened, and degeneration of the tissue considerably advanced.

(D) CONCERNING THE UPPER EXTREMITIES.

ART. 117.—*On the Suspension of the Radial Pulse in forced extension of the Arm.* By Dr. AV. VERNEUIL.

(*Journal de Physiologie*, July, 1858.)

Dr. Av. Verneuil has ascertained that the radial pulse is suspended whenever the arm is placed in a position of *forced extension*, and that this suspension is due to the compression of the brachial artery at the bend of the elbow by the aponeurotic expansion of the biceps and brachialis anticus. He has ascertained, indeed, that the same result is brought about by forcible extension of the arm, as is known to be brought about by forcible flexion of the arm. The practical suggestions arising out of this fact are obvious.

ART. 118.—*Dislocation at the Radio-carpal Articulation.*

By Dr. D. I. DUIGAN, Surgeon R.N., H.M.S. Vulture, Gibraltar.

(*Medical Times and Gazette*, Nov. 6, 1858.)

CASE.—J. C—, æt. 16, a sailor-boy of the second class, while employed, on July 29th, in making fast some washed clothes in the rigging, stood, contrary to orders, upon the clothes-line, which immediately gave way beneath him. He fell to the deck, a height of about twelve feet, and came partly upon his side, with the left hand and forearm beneath him: two handspikes, belonging to an adjoining gun, happened to be lying on the deck at the moment of the accident, and he dropped across them.

When raised, the forearm was in a semiflexed and semi-prone position; the hand was thrown backwards, and twisted as if on its axis; a depression existed on the radial side of the dorsal aspect of the wrist, and a tumour on the palmar, formed by the dislocated end of the radius, the outline of which could be distinctly felt in its new position. Knowing how generally force applied at the wrist is followed by fracture of the radius about its lower third (recognised in Dublin as Colles' fracture), and by a second variety (Dupuytren's) when fractured into the joint—the limb was submitted in this case to a very decided manipulation, before attempting reduction, in search of crepitus, or abnormal mobility; and on reducing the luxation, which was easily done, the same manœuvres were again employed without discovering any solution of continuity.

This injury is so seldom seen, that its occurrence has been almost doubted by some high foreign authorities, under the belief that the radius must fracture at its carpal extremity rather than dislocate. However, although this be the general rule, it has its exceptions, rare though they be. The result of violence applied to the wrist must naturally be influenced by the age and condition of the osseous and ligamentous structures, for the old and earthy bone—retained by its tough ligaments—would fracture, where a younger one, abounding in animal matter, and confined by less dense and more elastic fibrous tissues, would be saved by its flexibility.

Naval surgeons have one great advantage in all these casualties. The patient is seen without loss of time at the moment of injury—while labouring under faintness or shock, while all is relaxed, and long before effusion into the surrounding parts can mask the true nature of the accident, or oppose its reduction.

Dr. Fulton, of H.M.S. Centaur, has had a similar accident recently under his care; one of the officers having been thrown from his horse while riding at Malta.

PART III.

MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

(A) CONCERNING PREGNANCY AND PARTURITION.

ART. 119.—*On the liability to Abortion.*

By Dr. CLAY, Physician to St. Mary's Hospital, Manchester.

(*Glasgow Medical Journal*, Jan., 1859.)

DR. CLAY'S inquiry includes 790 females who passed under his notice in hospital and private practice. The combined age of these 790 females is 40,040 years, giving an average of $50\frac{1}{2}$ years to each, or rather more. Of these 790 females, 430, or considerably more than one half, have had living children, premature births, and abortions; 350, or considerably less than one half, have never aborted, but always had full-period living children; 10, or a 79th part of the whole of these, had neither dead nor living children, nor yet abortions. Of the whole 790 females, not one had abortions alone, viz., without having had living children at some time or other. The entire number of pregnancies (whatever the result) of the 790 females was 6970, or about $8\frac{1}{2}$ to each female. Of the 6970 pregnancies, 5970 were living children, and 1000 abortions. The number of living children to each female will be about $7\frac{1}{2}$, and the number of abortions to each about $1\frac{1}{2}$. The greatest number of pregnancies in any one female was 30. The greatest number of abortions 28. The last two numbers were of the same female; that is, 2 living children and 28 abortions. This female had been married twice. By her first husband she had only 2 living children; by the second husband she had 28 abortions, and no living children. Pursuing the subject as to the number of pregnancies—

1	had	30	pregnancies.
50	„	15	„
20	„	14	„
40	„	13	„
40	„	12	„
70	„	11	„
130	„	10	„

70	had	9	pregnancies.
80	„	8	„
60	„	7	„
50	„	6	„
50	„	5	„
50	„	4	„
The rest had 3, 2, 1, or 0 each.			

It will be observed in this table that the greatest number of females had each 10 pregnancies of all results; and that those of 11 and 9 pregnancies each were the next in amount.

The entire number of abortions, or children born dead, was 1000, or about $1\frac{1}{2}$ to each individual. Of these—

1 female had 28 abortions.				30 females had 3 abortions.			
60	”	6	”	80	”	2	”
10	”	5	”	272	”	1	”
10	”	4	”				

Showing by far the largest majority in those females with only 1 abortion; and next to those the females who had 2 each.

Dividing the child-bearing period into two epochs—the *first* from puberty to thirty years of age, and the *second* from the age of thirty to the cessation of menstruation, the following numbers in respect to abortion were obtained—

Before thirty years of age	460 abortions.
After thirty years of age	540 „

So that abortion occurs most frequently after thirty years of age, or in the latter half of the child-bearing period.

In the above statement it is remarkable, that no case occurs where a female has passed through life, and been capable of impregnation, but has had living children at some period of the term, though subject to frequent abortions; and there were only 10 who were not subject to impregnation at all, and therefore had neither living nor dead children.

In corroboration of Dr. Whitehead's views, Dr. Clay says, that in so far as his investigations have progressed, pregnancies are rare occurrences during the last three or four years of menstrual life. Out of 790 there were only

1 at the age of	52	4 at the age of	49
2 at the age of	50	5 at the age of	48

Indeed, from forty-one or forty-two years of age to the period of complete cessation, impregnation is far from being a common occurrence. The capacity of child-bearing is, in fact, on the decline for the last six years of the menstrual period.

The number of impregnations to each female, as deduced from the above tables, is about $8\frac{1}{2}$. Dr. Whitehead's investigations make it about 12 to each. Dr. Clay does not know how this discrepancy arises, whether from the yet small extent of these inquiries, and from their being confined to females who have passed through the full term of child-bearing life. The average age of Dr. Whitehead's 2000 cases being but *thirty* years, whilst that of Dr. Clay's 790 cases was *fifty and a half* years.

DR. CLAY.		DR. WHITEHEAD.	
Pregnancies of all results,	$8\frac{1}{2}$	Pregnancies of all results,	12
Living children	$7\frac{1}{2}$	Living children	6.37
Abortions	$1\frac{1}{2}$	Abortions	1.63

Lastly, it is to be observed, that the above tables show that more than one half the females under inquiry, or 430, are subject to abortion. Of 790, Dr. Whitehead makes abortion 37 per cent.; the tables here given show more than 50 per cent. These differences may, however, be easily accounted for, when it is considered that abortions are most frequent in the latter half period of menstruation, and the average age of Dr. Clay's tables being *fifty and a half* years, whilst that of Dr. Whitehead's is only *thirty*.

In conclusion, Dr. Whitehead believes that the frequency of abortion rests with the third, fourth, and subsequent pregnancies, whilst Dr. Clay's tables show that abortions are most frequent in the latter half of menstrual life, which views are in some measure analogous.

ART. 120.—*Beneficial effects of Pepsine in the obstinate vomiting of Pregnancy.* By Dr. LE GROS.

(*Bull. Gén. de Therapeutique*, Feb. 15, 1858; and *Med.-Chir. Review*, July, 1858.)

In the great majority of cases the vomiting of pregnancy may safely be left to the influence of time; but there are some cases in which females are scarcely able to retain in their digestive system a sufficient amount of nourishment to support their existence, and are therefore reduced to the last degree of emaciation. In some, also, the shocks occasioned by this obstinate and repeated vomiting become the source of abortions, which might have been prevented by moderating the activity of the morbid phenomenon. A very remarkable case was related in 1856, by M. Teissier, Professor of Clinical Medicine at Lyons, showing the immediately beneficial effect of a dose of pepsine in a case of vomiting during pregnancy. In this case the symptoms resisted all the ordinary methods which were employed, and the patient was unable to retain in her stomach any substance whatever. Under these circumstances, the patient was brought to M. Teissier, who found her in the following condition: the vomiting had continued for two months, and she was at the end of the fourth month of her pregnancy; she presented the appearance of a skeleton, having the aspect and the cough of a phthisical subject; the pulse was 140, and M. Teissier thought at first that the case was one of pulmonary tubercle. Finding that all treatment had been hitherto inefficacious, and that the lady was actually dying of inanition, he was seriously meditating upon the propriety of inducing abortion as a means of saving her life: but as a last resource before operating, he determined to employ pepsine. He accordingly prescribed one gramme, to be divided into two doses, and taken every day in a spoonful of broth. At the very first dose the broth was retained, and from that moment the vomiting never returned. On the third day the lady ate some chicken, and then some beef-steak. The treatment was continued in the same manner for three weeks, and at the end of that time the cure was complete: the emaciation was replaced by *embonpoint*, the fever and the cough ceased with the vomiting, and at the end of the ninth month the lady was safely delivered.

Dr. Gros then relates six other cases in which the pepsine was em-

ployed with the same success, and he thinks himself warranted in concluding that pepsine undoubtedly produces good effects in the vomiting which attends pregnancy. He explains the results by supposing that, although in the first instance the vomiting is due only to the sympathy existing between the uterus and the stomach, yet subsequently the stomach itself becomes affected, as is proved by the fact that, in the beginning of pregnancy the vomiting occurs only in the morning or the evening; but in aggravated cases it supervenes after every meal, and all alimentary matters are rejected. In such cases, therefore, when the stomach has taken on a morbid habit, and exhibits an alteration of secretion, the pepsine appears to be really indicated; although in a merely sympathetic action between the uterus and stomach it would be difficult to explain the efficacy of its action.

ART. 121.—*On the removal of the Placenta in the early months of Pregnancy by evulsion.* By Dr. O. C. GIBBS.

(*American Medical Monthly*, Dec., 1858.)

In a paper on this subject Dr. Gibbs says—"The polished and convex surface of Dr. Carey's decidual separator is admirably adapted to glide along the inner surface of the womb, and with a little management, to separate the placental attachment. We now use it *without delay* for the removal of retained secundines, after abortion, *when any considerable hæmorrhage is present*; and we have seen, and can see, no possible danger from its judicious use. Instead of making it, in such cases, our *dernier*, we make it our first resort. We have little doubt but that the uterus will invariably contract, on the removal of the secundines *in abortion*, yet from habit we almost invariably give ergot."

ART. 122.—*Watery discharge from the Uterus during Pregnancy.*
By Dr. HARVEY.

(*Dublin Quarterly Journal of Med. Science*, Feb., 1858.)

Dr. Harvey made the following remarks on this affection at a meeting of the Cork Med. and Surg. Soc., Dec. 9th, 1857: "In some books on Midwifery, watery discharge from the uterus is noticed as amongst the diseases to which pregnant women are liable. A clear, limpid, colourless fluid, oozing in quantity from a few ounces to pints daily, flows away, sometimes stopping for a short time, and recommencing; and in the majority of cases it continues nearly, or full to the time of delivery. The abdomen does not appear palpably reduced by these discharges, and a living child is commonly born at or near the full time. In the greater number of instances also, there is evidence of the usual quantity of liquor amnii being present on the supervention of labour. Dr. Alexander's case, given in the third volume of the 'Medical Commentaries,' shows this very prominently. In a case by Dr. Petel, also, in the 'Gazette de Hôpitaux' of July, 1838, the liquor amnii is specially mentioned as normal in quantity.

"What is the source of this fluid, discharged as it is, to the amount

of hundreds of pints in the course of a few months? The supposition of its coming from the cervical glands of the uterus, or from the vagina, both of which have been assumed as sources of it by different authorities, appeared altogether unlikely from the nature of the fluid, its quantity, and its mode of coming away in gushes of a considerable quantity at a time. That it could come from the space between the decidua and chorion, or between the chorion and amnion, we have no pathological facts, so far as I am aware, to warrant our supposing such a source for the flow; whilst in the natural condition of parts such spaces do not exist; as, at a period of pregnancy before these discharges commonly show themselves (say the sixth month, or thereabouts), the cavity between the chorion and amnion has disappeared; and we know that the chorion and decidua are in contact throughout.

"Under these circumstances we seem driven to the conclusion that the amnion must be the source of this flow; that there may be occasional solution of continuity in this membrane, admitting of discharges from time to time, which either close again, or admit by the mechanical relations of the bag to the neighbouring parts of the amnion, refilling to a certain extent by a fresh secretion of its particular fluid. In confirmation of this view may be mentioned cases recorded by Dr. Denman, Professor Burns, of Glasgow, and Dr. Pentland, of Dublin, in which the amnion is said to have given way from fright or other sudden shock, the water being discharged without labour coming on. All these considerations tend rather to the view that the escaping fluid may be liquor amnii than to any other which has been propounded. In the case which I am going to relate, the symptoms were similar to those which were present in the cases of watery discharge which I have been noticing, and in this instance, as will be seen, the flow was undoubtedly amniotic.

"Mrs. ———, mother of several children, was for more than a year the subject of heavy sanguineous discharges, which were so little influenced by the treatment adopted, that the existence of polypus was thought possible. An examination revealed considerable congestion of the os and cervix uteri, with superficial ulceration, which gave way to treatment generally and locally applied. During last summer her health was considerably improved, but occasionally menorrhagic attacks, which latterly observed more or less closely the monthly periods, showed themselves. Matters were going on thus when she suffered a considerable shock by her eldest boy meeting with a severe accident, in which his arm was fractured. On that day, for the first time (six weeks before delivery), she had a sudden gush of clear watery fluid from the vagina, and since that time to the date of these notes (5th November), she was scarcely free from it; it would diminish or nearly stop for a few days at a time, to come on again in gushes, and in considerable quantity. The quantity escaping in one of these was seldom less, and generally more, than half a pint, and on the late occasion, when the flow was accompanied by a heavy sanguineous discharge also, she thinks the combined amount was fully a quart. It came on in the horizontal position as well as in the erect, and apparently without any cause. The size of the abdomen did not appear much affected by these at any time.

"The occurrence of the watery discharge suggesting the probability of pregnancy, notwithstanding the menstrual changes which had been going

on with some regularity, and that, if pregnancy did exist, the ovum might have suffered hydatid degeneration, I proposed an examination for the purpose of ascertaining the point. I found an abdominal tumour, occupying the hypogastrium to above the umbilicus, and on laying my hands over its surface, it gave a good example of the value of a diagnostic indication lately suggested by Dr. Oldham; it afforded distinct evidence of its being uterine by gradually and regularly hardening under my hand. The movements of the child were also felt, and foetal pulsation, distinctly heard by the stethoscope, put an end to all doubts.

"I told the lady that she had passed some six or near seven months of her pregnancy without being aware of it, and that her labour would probably come on prematurely, all of which she entirely disbelieved, and I could not induce her to make the necessary preparations. Two days after, I was called to her—the first stage of labour having set in with unusual distress and irritation; the pains particularly sharp and unbearable; the os uteri was hard and unyielding, and the breech, presenting in the second position, was felt in close contact. I immediately put her on antimonial solution, notwithstanding which the os uteri took over three hours to relax. After a first stage of about four and a half hours, and a second of less than half an hour, a male child, of scarcely seven months' growth, was born. The presenting hip and buttock were perfectly black, evidently from the direct pressure to which they had been subjected, in consequence of the loss of the liquor amnii. None whatever escaped with the child, and the sanguineous discharge was also unusually scanty. I do not think I ever witnessed so dry a labour."

ART. 123.—*On the use of Chloroform during labour.*

By Dr. SPIEGELBERG.

(*Monatsch. für Geburtsh.*, Bd. xi, pp. 29—34; and *Medical Times and Gazette*, March 5, 1859.)

At the Berlin Midwifery Society, Dr. Spiegelberg, noticing the chief objections which have been offered to the employment of chloroform during labour, observed that one of the chief of these was that it delayed, weakened, or put off the pains. But not only was there no physiological reason why it should act thus, but all practical observation contradicted the assertion. No one had seen such effects when the anæsthetic had been properly administered; and even when the narcosis has been carried to the complete annihilation of sensibility and voluntary movements, after a temporary suspension, the pains return quite regularly again at the end of a few minutes. Many practitioners only measure the degree of the activity of the pains by the cries and movements of the patient; and as these disappear, they believe that the pains also have disappeared, or at least have become much enfeebled. That the voluntary activity of the abdominal forces has disappeared is true enough; but the involuntary action, which alone is quite sufficient, remains unimpaired. A second objection to the use of chloroform is still less founded, viz., that which attributes mischief to the mother consequent on its employment; and by the unprejudiced mind the practice of those who employ chloroform will be found to be more successful than that of those who reject it. As yet no case of death has occurred from

its obstetrical employment. Dr. Spiegelberg believes that it is admissible, though not necessary, in cases of entirely normal labour, and in these he is guided by the wishes of the patient; while in abnormal, and especially in instrumental labour, it is urgently called for when contra-indications do not present themselves. Certainly no innovation in practice of such importance has ever met with such rapid approval as the obstetrical use of chloroform; and a considerable portion of the opposition it has met with has been due to the faulty mode of its employment. To secure its beneficial agency the patient should be kept in a state of complete repose, and brought at once into a pretty deep state of anæsthesia, no stage of excitement then occurring. Afterwards she should only inhale during the presence of the pains, and to an extent only necessary to relieve these. The simplest form of apparatus should be used, namely, a conically folded handkerchief.

Dr. Schneemann observed that he could not agree in the desirableness of chloroforming in ordinary cases, inasmuch as there was no necessity for it, while he had often observed ill consequences, especially a disturbance in the delivery of the placenta; and, moreover, the maintenance of the anæsthesia is a difficult and time-consuming procedure when no assistant is present.

Dr. Birnbaum had never met with any of these ill consequences. He resorted to chloroform in operative midwifery, and in dynamic disturbances when appearing to be dependent upon excitement of the nervous system.

Dr. Breslau objected to the giving the chloroform on a handkerchief, because too much is used in this way, and the air of the room becomes impregnated with it. He employs, usually, a caoutchouc bottle, containing a sponge soaked in chloroform.

Dr. Birnbaum objected to all such means, for the escape of the chloroform, by due care and the proper folding of the handkerchief, may be prevented.

Dr. Spiegelberg observed, that although certainly more chloroform is employed when the handkerchief is used, yet this deserves to be preferred. When an inhaler is used, we cannot, with the same ease as with the handkerchief, follow the various movements of the head. Then again all these apparatus have something repulsive about them, as they have to be used by one person after another; and it is inconvenient carrying them about one. But the greatest objection is that they cover up the mouth, and sometimes the nose also, which is productive of danger.

Dr. Kilian observed that he employed chloroform in normal labour only in very restless and very sensitive persons, especially in the last stage, when the perinæum is endangered by the restlessness. In operative midwifery, except in the Cæsarean section, when relaxation of the uterus and hæmorrhage are to be feared, and in dynamic disturbances, he, at the present time, makes a pretty extensive use of anæsthesia. He only induces the so-called first stage of narcosis by applying a handkerchief to the face, enabling the air as well as the chloroform to come in contact with it, and repeating this with the recurrence of the pain. He has never met with any arrest of uterine activity, or any trouble in the delivery of the placenta.

ART. 124.—*On the shortening of the Duration of Labour.*
By Dr. J. GRAY.

(*Glasgow Medical Journal*, Jan., 1858.)

In order to increase the action of the uterus, and thereby hasten delivery, Dr. J. Gray advises us to excite the nipple as labour-pain comes on, and continue the stimulation so long as it lasts. This is accomplished by passing the left hand gently but continuously upwards and downwards over one or other of the nipples; or by simulating with the fingers the act of sucking of the infant. By such manipulation, he says, the nipple erects, and, in virtue of *reflex action*, the uterine contractions increase in force; while at the same time the os dilates, and the external parts become relaxed. Besides shortening the duration of labour, he finds it has also the effect of preventing hæmorrhage. The second stage of labour completed, if the placenta be not in the passage, he still maintains at short intervals the friction over the nipple, in order that the uterus may expel its contents; and also resorts to it in cases where he has occasion to fear "flooding."

"Reasoning," Dr. Gray remarks, "from the practice of ancient and modern times, and influenced especially by the theory of reflex action, as recently discovered and propounded by Marshall Hall, I have lately been led to adopt this mode of irritating the nipple, in order to increase the action of the uterus, with a view to the abridgement of labour. If, I inquired, the application of the child to the breast causes the womb forcibly to contract, and thus prevents 'flooding,' may not a similar operation, artificially performed, have the same effect in promoting the contractile efforts of the uterus, and hastening the delivery? That it does so, very ample trial has fully convinced me. In difficult cases, indeed, which require instrumental aid for their termination, it will most probably prove useless—failure depending upon malformation either on the part of mother or child. But where the child is in the normal position and of average size, and should no deformity exist in the pelvis or soft parts of the mother, I have found it generally successful. I never, however, be it remembered, interfere in those cases where there is already *active* uterine contraction."

ART. 125.—*On the prevention of Laceration of the Perinæum.*
By Dr. MATTEI.

(*Vierteiljahrsschr. f. Prakt. Heilkunde*, 1858; and *Medico-Chir. Review*, Oct., 1858.)

Dr. Mattei has the following remarks on the means of preventing laceration of the perinæum. It is especially necessary that the head pass the vulva in a favorable direction. This can only happen when it passes with the necessary degree of flexion. While the occiput passes under the pubic arch, the face has not yet quitted the pelvic outlet; first when the upper part of the neck comes under the pubic arch, can the extension of the head (or the separation of the chin from the breast) begin. If the distension of the perinæum begins too early, the head must pass the vulva with unfavorable diameters—namely, with the great

oblique, or great or straight diagonal diameters. Such a passage easily causes laceration. Hence it is the task of the physician to prevent a premature distension by the head. This he effects by placing two fingers between the labia, or in some cases between the pubic arch and occiput, so as to bring the head downward and outward, at the same time laying the other hand on the hinder part of the perinæum, upon which the face is lying, and pushes this upward. This manœuvre is to be executed during the pains, which will thus protrude the head forward in the requisite arc. A very simple means of expediting the birth of the head consists in compressing firmly the distended perinæum with the whole hand. This resembles the squeezing out of the kernel from a cherry. On the passage of the shoulders care must also be taken lest the two shoulders pass together.

ART. 126.—*On the pathology and treatment of Placenta Prævia.*
By Dr. A. S. DONKIN, of Newcastle-upon-Tyne.

(*Edin. Med. Journal*, April, 1859.)

Dr. Donkin's object in this paper is to show that in those cases in which the placenta is detached and the hæmorrhage arrested by nature's spontaneous effort, *the separation of the placenta and the arrest of the flooding do not stand in the relation of cause and effect, but as the concomitant result of cervical expansion; both progressing pari passu. Nature then does not separate the placenta completely from the cervix, until the very period arrives when she has completed the mechanical closure of the mouths of the utero-placental vessels, which have been opened in the process.* The forcible detachment of the placenta by the finger, to any extent, is, therefore, *incorrect in principle*, inasmuch as it merely tears off the placenta, without making any provision for arresting the hæmorrhage, which flows from the vascular orifices opened on the cervix by the operation.

The practical objections to which artificial detachment of the placenta is liable, as proved by experience, are the following: *First*, the gross mortality in those cases in which it has been employed is equivalent to one in four sixteenths; while the gross mortality in cases of spontaneous expulsion of the placenta is only one in fourteen and a half. *Secondly*, that although introduced into practice for the ostensible purpose of preventing the necessity of turning in a certain class of cases, we find that exactly *one half* of the cases treated by this method subsequently required the operation of turning in addition.* Notwithstanding these objections, the fact must not be ignored, that in a considerable number of cases a cessation of the hæmorrhage has followed this practice,—a result which appears in many instances to have saved the life of the mother. But as the operation is attended and immediately followed by a profuse flow of blood, this, by suddenly and powerfully depressing the heart's action, will permit the process of coagulation to take place in the

* In Dr. Trask's Statistical Table III, we find that, up to the period of its publication in 1855, the total number of cases on record in which the placenta was forcibly detached by the finger amounted to 66: of these, 47 recovered and 13 died; while 33, or exactly half the number, required the operation of turning in addition.

bleeding mouths of the vessels. In this manner we may account for the subsequent arrest of the flooding; for we know that a sudden and copious gush of blood, by its salutary influence in producing early and temporary prostration or collapse, and thereby enabling coagula to form, is considerably less dangerous to life than a small and continuous stream of blood, which seldom acts on the circulation until it does so with a force which is at once irretrievably fatal. Thus, in thirty-one of the recorded cases, or about one half of the entire number treated by this method, it is expressly stated that the detachment of the placenta was resorted to under the condition of extreme exhaustion.* Now in these cases the already existing prostration of the circulation was unquestionably such as would enable coagulation to plug up the open mouths of the utero-placental vessels, and thereby prevent further bleeding. In other cases, again, it would seem that the operation was performed at the period when nature would have detached the organ; so that in these cases the arrest of the flooding can be accounted for by the change which the cervix had already undergone.

So far, then, as our statistical knowledge will enable us to judge of the value of this method of treating *placenta prævia*, it would appear not to have been attended with the success which was anticipated on its first introduction into practice; consequently, we are justified in attempting to devise some other means of arresting the flooding in the early stage of labour, in those cases whose characters rank them in the second class already defined. If we desire any method of treatment to be successful in these hazardous cases, we ought to endeavour to base it on a correct appreciation of the process which nature brings into operation to arrest the hæmorrhage. If, therefore, it is correct that the process in question is *expansion of the cervix*, we ought to assist her in effecting it, when her own powers are inadequate for the purpose.

It is from these considerations that, in the cases alluded to, in which something must always be done not only to check the flooding but to advance the labour, that *mechanical expansion of the cervix, by means of a sponge tent specially constructed for the purpose*, is recommended as a method of treatment. From the operation of this procedure the following results may reasonably be expected:

1. It would gradually throw off the placenta, and by putting the fibrous structure of the cervix on the stretch, it would compress the utero-placental vessels. In other words, it would, by its action on the cervix, detach the placenta and arrest the hæmorrhage *pari passu*.

2. It would act both as a plug and as a powerful compress applied to the opened mouths of the utero-placental vessels.

3. It would excite uterine action.

In order to produce these important effects, the *sponge tent* employed would require to be rounded at its upper extremity to prevent its introduction injuring the placenta; it would require to be of large size, and so constructed as to expand rapidly under the influence of tepid injections.

To this method of treatment might be added the administration of ergot, or the application of galvanism, as recommended by Dr. Mackenzie, of London, according to the peculiarities of each individual case.

* Of these cases 23 recovered and 11 died, giving a mortality of more than 1 in 3, or exactly 1 in 2½.

ART. 127.—*A case of Cæsarean section, with observations.*

By Dr. MURPHY, Professor of Midwifery in University College, London.

(*Dublin Quarterly Journal of Medicine*, Feb., 1859.)

Little need be said about the case. The patient was thirty years of age, and the mother of seven children, all of whom had been born at full time without difficulty. The cause was distorted pelvis from mollities ossium. The operation was performed on the 12th of July, 1859, and a dead child extracted; death happened from asthenia on the 14th.

Having no faith in conclusions derived from the statistics of the Cæsarean operation, Dr. Murphy has endeavoured rather to compare similar cases, and the results of each operation, and the inquiry has led him to these conclusions:

"1st. That there are certain cases of mollities ossium in which it is impossible to extract the child; in others, it may be possible to do so, but by an operation of such difficulty and danger to the mother, that we are not justified in sacrificing human life for such a doubtful chance.

"2dly. There are cases of distortion, in consequence of rickets, in which the disproportion is confined to the antero-posterior measurement of the brim of the pelvis; but that space is sometimes so diminished, that the operation of craniotomy becomes equally difficult and dangerous as in the former case. Under these circumstances, the same rule applies with even more force; because, as the woman is generally in good health, the chances of recovery from the Cæsarean section are greater. This operation seems to me justifiable, if the conjugate axis of the brim is only two inches.

"3dly. When tumours obstruct the pelvic cavity so as to leave a space of little more than two inches through which to extract the child, the practitioner is not justified in attempting craniotomy; not only because of the danger to the mother of the operation itself, as in Dr. Shekleton's case, but because the pre-existing disease in the parent renders her life so doubtful that we are not justified in taking human life when there is every probability of the mother sinking under the disease, even if she escape the dangerous operation of craniotomy.

"Lastly. I may add, that my objection to statistical conclusions is founded on the fact—1st. That the number of cases in which the Cæsarean section has been performed in these countries is not sufficiently numerous to correct the errors produced by accidental causes. The operation has been performed in several cases under every disadvantage, arising from long-protracted labour, pre-existing inflammation, and such like causes of a fatal issue, the mortality being thereby disproportionately increased. 2dly. It is at present impossible to separate those cases in which craniotomy was performed, in consequence of extreme disproportion in the pelvis, from those in which the disproportion is only so great as to prevent delivery by the forceps. Consequently, the total results of such cases must be erroneous. With this objection, I shall place in a tabular form the whole number of cases reported."

Place.	Total cases.	Mothers.		Children.		Observations.
		Living.	Dead.	Living.	Dead.	
Great Britain	57	10	46	34	25	Cause of death in Mr. White-head's case doubtful. Two results not reported. Dr. West's Table.
America . .	12	8	4	6	4	
Europe . .	409	158	251	237	110	
Total .	478	176	301	277	139	

British cases of Cæsarean section.

No.	Year.	Practitioner.	Cause.	Duration of labour.	Mothers.		Children.	
					Living.	Dead.	Living.	Dead.
1	1737	Mr. R. Smith .	Mollities ossium	7 days	...	1	...	1
2	1739	Mary Donnelly	12 „	1	1
3	1740	Dr. White	1	...	1
4	1769	Mr. Thompson	1	...	1
5	1773	Dr. Young . .	Rickets	1	...	1
6	...	Mr. A. Wood	1	...	1
7	1774	Mr. Chalmers	12 „	...	1	1	
8	1774	Mr. John Hunter	Mollities ossium	1	1	
9	1774	Dr. Cooper	2 „	...	1	1	
10	1775	Mr. W. Whyte .	Ditto.	1	...	1
11	1777	Mr. Atkinson .	Ditto.	3 „	...	1	1	
12	...	Mr. Clarke	8 „	...	1	...	1
13	1793	Mr. Barlow	5 „	1	1
14	1794	Dr. Hull . . .	Ditto.	1	1	
15	1795	Dr. Hamilton, Jr.	Ditto.	2 „	...	1	1	
16	1798	Dr. Hull . . .	Ditto.	10 „	...	1	...	1
17	1798	Mr. Kay	3 „	...	1	1	
18	1799	Mr. Wood . .	Ditto.	1	1	
19	1800	Mr. John Bell	1	1	
20	1801	Mr. Dunlop . .	Ditto.	1	1	
<i>Forward</i>					2	18	10	10

AUTHORITIES.

CASE.

- 1 Smellie, vol. iii, p. 422.
- 2 Edinburgh Essays, vol. v.
- 3 Hull's Defence, p. 67.
- 4 Medical Observations, vol. iv.
- 5 Manuscript Lectures, by Dale.
- 6 Hamilton's Outlines.
- 7 Ditto ditto.
- 8 Medical Observations, vol. v.
- 9 Ditto ditto.
- 10 Hull's Defence.

CASE.

- 11 Hull's Defence, p. 67.
- 12 Mem. Med. Society, vol. iii.
- 13 Medical Records, p. 154.
- 14 Hull, p. 172.
- 15 Hamilton's Outlines.
- 16 Hull, p. 162.
- 17 Ditto.
- 18 Mem. Med. Society, vol. v.
- 19 Medico-Chirurgical Transact., vol. iv.
- 20 Appendix to Hull's Translation.

No.	Year.	Practitioner.	Cause.	Duration of labour.	Mothers.		Children.	
					Living.	Dead.	Living.	Dead.
				<i>Forward</i>	2	18	10	10
21	...	Mr. Wood	1	...	1
22	1817	Barlow and Cost	1	1	
23	1820	Dr. Radford . .	Mollities ossium	34 hours	...	1	...	1
24	1821	Dr. Radford* .	Ditto.	1	...	1
25	1821	Barlow & Dugdale	1	1	
26	1821	Henderson	1	1	
27	1825	Dr. Radford† .	Ditto.	83 „	...	1	2	
28	...	Dr. Radford	53 „	...	1	...	1
29	1826	Dr. Crichton	6 days	...	1	1	
30	1827	Mr. Knowles . .	Ditto.	30 hours	1	...	1	
31	1829	Dr. M'Kibbins .	Exostosis	1	...	1
32	...	Mr. Ward‡	1	...	1
33	1833	Mr. Greaves	1	...	1	
34	1834	Dr. Montgomery	Fibrous tumour	1	...	1
35	1841	Mr. Ross	1	1	
36	1843	Dr. Elliot	1	...	1
37	1843	Mr. Goodman and Dr. Radford .	Mollities ossium	5 days	1	...	1	
38	1840	Mr. Whitehead§	Ditto.	1	
39	1843	Mr. Braid	1	...	1
40	1843	Bailey and Hardy	1	2	
41	1845	Mr. Lyon	1	1	
42	...	Dr. Wright¶	1	...	1	
43	1847	Mr. Skey . . .	Rickets	1	1	
44	1849	Dr. Radford . .	Mollities ossium	1	...	1	
45	1849	Mr. Campbell	1	1	
46	1850	Mr. Nimmo	1	1	
47	1850	Dr. Sannerman	1	...	1
48	1850	Dr. West . . .	Ditto.	1	1	
49	1850	Dr. Oldham . .	Ditto.	1	...	1
				<i>Forward</i>	7	41	30	21

AUTHORITIES.

CASE.

- 21 Medical and Phys. Journal, p. 346.
 22 Barlow's Essays.
 23 Edinburgh Med. Journal, vol. lv, p. 67.
 24 Ditto ditto.
 25 Merriman and Churchill, p. 317.
 26 Ditto ditto.
 27 British Medical Journal, p. 45, 1856.
 28 Ranking, vol. x, p. 212.
 29 Edinburgh Journal, 1828.
 30 Transact. of Prov. Assoc., vol. iv, p. 53.
 31 Ditto ditto, 1831, p. 352.
 32 Lancet, vol. ii, 1839-40, p. 28.
 33 Lancet, 1833, p. 148.
 34 Dublin Journal, vol. vi, p. 418.
 35 Edinburgh Monthly Journal, 1842.

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- 36 Churchill's Midwifery, p. 321.
 37 British Obstetrical Record, vol. i.
 38 Medical Gazette, 1841, p. 940.
 39 Ranking, vol. vii, p. 330.
 40 Ditto ditto.
 41 Edinburgh Medical Journal, Dec. 1845.
 42 Lancet, vol. ii, 1839-40, p. 28.
 43 Ranking, vol. v, p. 293.
 44 Ditto vol. x, p. 212.
 45 Ditto vol. x, p. 330.
 46 Edinburgh Monthly Journal, 1850.
 47 Lancet, July, 1850.
 48 Med.-Chirur. Trans., vol. xxxiv, p. 61.
 49 Ditto ditto, p. 89.

* The child's head caught by the contracting uterus caused its death. + Twins.

‡ One of six cases quoted by the Lancet from 'L'Experience,' but the details not given.

§ Mother died on thirty-second day, of hip-joint disease.

|| Twins.

¶ Quoted by Lancet from 'L'Experience,' vol. ii, p. 28. 1839-40.

No.	Year.	Practitioner.	Cause.	Duration of labour.	Mothers.		Children.	
					Living.	Dead.	Living.	Dead.
				<i>Forward</i>	7	41	30	21
50	1851	Dr. Oldham . .	Scirrhus tumour	1	...	1	
51	1853	Dr. Waller . .	Fibrous tumour	1	1	
52	1854	Dr. Simpson* .	Mollities ossium	1	...	1
53	1856	Mr. Humphrey	1	...	1
54	1856	Mr. Thornton	1	1
55	1858	Dr. Greenhalgh .	Ditto.	1	1	
56	1858	Mr. Hawkins .	Rickets . . .	1 hour	1	...	1	
57	1858	Dr. Murphy . .	Mollities ossium	1	...	1
					10	46	34	25

American cases of Cæsarean section.

1	1822	Mr. Cellin	1	...	1	
2	1827	Dr. Richmond, O.	1	...	1	
3	1827	Drs. Dougal and Vanvalsah†	1	...	1
4	1835	Messrs. Nacrede and Gibson	1	...	1	
5	1837	Dr. Fox and Mr. Gibson	1	...	1	
6	1845	Dr. Brodie Herdon	1	1P.
7	1843	Dr. Cyrus Falconer‡	1	1	
8	1848	Dr. A. B. Shipman§	A tumour	1	...	1
9	1850	Mr. M. H. Jetter	1	1
10	1851	Dr. W. H. Merinar	1			
11	1851	Ditto.	1			
12	1851	Ditto.	1	1	
					8	4	6	4

AUTHORITIES.

CASE.

- 50 Lancet, vol. ii, p. 226, 1851.
 51 Med. Times and Gaz., vol. xxvii, p. 266.
 52 British Medical Journal, 1854, p. 1066.
 53 Ditto ditto 1856, p. 779.

CASE.

- 54 Lancet, 1857, p. 313.
 55 British Medical Journal, 1858, p. 377.
 56 Medical Times and Gazette, 1858, p. 481.

AUTHORITIES FOR AMERICAN CASES.

CASE.

- 1 New York Journal, March, 1822.
 2 West. Medical Journal, Nov. 1827.
 3 American Journal of Medical Sciences, Old Series, vol. xvi, p. 346.
 4 Ditto ditto p. 343.
 5 Ditto ditto vol. xxii, p. 13.
 6 American Journ. of Med. Science, N. S. vol. xii, p. 386.

CASE.

- 7 American Journ. of Med. Science, N. S. vol. vi, p. 264.
 8 American Journal, vol. xviii, p. 122.
 9 Ditto ditto, vol. xxi, p. 538.
 10 Ditto ditto, vol. xxxi, p. 567.
 11 Ditto, and Charleston Medical Journ. for March, 1851.
 12 Ditto ditto.

* Mother moribund before the operation.

+ Uterus ruptured before the operation.

† A dwarf, 3½ feet high.

§ Patient sinking before the operation.

|| Cases 10, 11, and 12—the same patient, who was delivered successfully twice by the Cæsarean section, previous to the third operation, from which she did not recover. Nothing is said about the previous children. Case reported by Dr. F. Owen.

ART. 128.—*On Delivery of the Child by Turning as a general rule in labour.* By Mr. E. GARLAND FIGG, of Borrowstowness.

(*Med. Times and Gazette*, Nov. 13 and 20, and Dec. 25, 1858.)

In this paper Mr. Figg attempts to show, not only that delivery by turning is preferable to delivery by the forceps in cases requiring operative interference, but that turning is the rule to be adopted in general cases. He tells us that he has attended sixty labours since writing these papers, that only *three of these were conducted as head presentations, and that of the remainder two were breech presentations, and fifty-five delivered by turning.* As the results of this astonishing practice we leave Mr. Figg to speak for himself:

“With regard to the children, they are generally still from two to five minutes, and in some cases half an hour’s duration. In many instances the first arm brought down is a little painful when moved for a day or two. I confess with humility that I have even broken four arms, which, though they occurred in cases of great pelvic contraction, were attributable to my own mismanagement in pressing over the shaft of the os humeri instead of following its line to the elbow. Should you commit the same error, with similar result, be not too candid to the relatives, but at once by your dictum transubstantiate the injury into a slight sprain received by the infant striking his shoulder against the backbone of the mother while actively prosecuting his uterine gambols. It will pass current, more especially if you appeal to her experience, when it is sure to be corroborated by a quotation of the day and hour of occurrence. Two slips of pasteboard applied, with a strip of calico a yard long, remedies the evil in ten days.

“In establishing a comparison between the advantages derivable from turning in primiparæ and multiparæ, I believe there is a preponderance of argument in favour of the former. In a primipara the os uteri is more in the axis of the pelvic brim, the body of the organ being more inclined to the perpendicular, and not projecting anteriorly, as in the frequent parturient; hence, in the former case, the uterine efforts of the last month previously to labour lodge the os and cervix inclusive of the head low in the cavity of the pelvis, not only assuring the practitioner by tangible proofs of the perfect capacity of the brim, but also presenting the best arrangement for the co-operation of the uterus with his extractive efforts. In the latter case, from the yielding of the abdominal muscles in former labours, the fundus bearing forwards, throws the os in the direction of the spine, rather than the pelvic cavity. Hence until the contraction of these muscles in some measure restore the proper axis, no advance can take place.

“The advantage in the second particular is briefly explained, by stating that in a primipara the antagonistic force is directly in line with the extractive. In a multipara it is entrenched round a corner.

“Again, in a primiparal case you have good grounds for the conviction that, in obviating the perineal stage, you limit the labour considerably; while in the latter patient an hour’s suffering might conclude the case.

“Be they right or wrong, these are the sentiments which have guided

my conduct at a large majority of my cases latterly, experience appearing to justify in happy results what theory dictated on sound reasoning. I hope I shall soon lose all mental impressions of a head lingering on the perinæum, or stationary from failing pains for hours. My primiparal patients are up in four days, without swelling of the vaginal muscles, nymphæ or labia; and what to me is perfectly unaccountable, with very slight laceration of the perinæum.

"I have had but one maternal death where the infant was turned, and that occurred five days after the event, by inflammation of the peritoneum of a patient, who, with contracted pelvis, had submitted to the ordeal to produce her sixth full-timed dead child.

"If I be entitled to any credit at all, it is for the candid avowal of a practice, that some, under fear of professional censure, would have adhered to but concealed.

"The operation was ancient, but nearly obsolete, and its revival by Dr. Simpson in particular circumstances led to my adoption of it in general cases."

In a latter communication, written chiefly as an answer to the strong objections of Drs. Robt. Lee, Ramsbotham, and Oldham, are the following passages: "Permit me," says Mr. Figg, "with humility to observe, that while physiology, anatomy, and analogy enable me to concoct as rational a theory for the operation as they can against it, I bring forward a formidable ally to my cause in nearly eighty-seven consecutive cases of perfect convalescence in mother and child, without adverting to a still greater number of successful instances effected at various intervals antecedently. Do these gentlemen impugn my veracity? Let them depute any member of the profession resident in Edinburgh, Glasgow, or London, to visit the locality of my residence, and by impartial inquiry of my patients prove its immunity from danger and their satisfaction as to its adoption." And again: "While my deliveries average two per week, I have had but one death during the year—the second child of a woman aged 45, born to a second husband after a widowhood of fourteen years."

ART. 129.—*A case of Delivery after the death of the mother.*

By Dr. FRENTHROP.

(*Gaz. Héb. de Med. et Chir.*, Jan. 21, 1859.)

CASE.—A young and strong peasant, who, on a previous occasion, had been delivered of a child without any difficulty. When Dr. Frenthrop saw her, she had been in labour upwards of forty hours; the waters had come away, and a sage-femme, finding that the labour was not progressing favorably, had made several fruitless attempts to deliver by turning. On examination, the right arm of the child was found protruding from the vagina, and the contractions of the uterus were fierce and incessant, although bleeding and other measures were employed to subdue them. Twenty-four hours later, the woman died undelivered. Some hours later still, she was undressed and laid out upon a straw-bed. On the day following, when arrangements were being made for the funeral, the child was found between the thighs of the mother, and the placenta was half expelled from the uterus. These are the principal facts in this remarkable case.

ART. 130.—*On the Puerperal Diseases observed in the Charité of Berlin.*
By Professor VIRCHOW.

(*Monatsch. f. Geburtskunde*, vol. xi, p. 409, 1858.)

Puerperal diseases had prevailed in the Charité, without cessation, from the autumn of 1856 to the middle of March, 1858, during which time eighty-three fatal cases occurred. As in the epidemic of 1846-47, the frequency and severity of cases was greater in winter than in spring, a circumstance in which puerperal fever differs from hospital gangrene, which occurs particularly in spring. According to the author's opinion, it is possible that this aggravation is owing to a concentration of the miasmata in the wards, ventilation being only sparingly used, for fear of producing colds.

In two cases only the very insignificant local changes were in no proportion to the severity of the symptoms; but it must be remarked, that these cases did not coincide with the maximum intensity of the epidemic, and that both patients had already, previous to their confinement, suffered with nervous symptoms. The rapidly fatal termination was thus not attributable to an intense alteration of the blood, but perhaps to a special predisposition of the nervous system. Acute endocarditis was observed in a large number of cases; it affected particularly the mitral valve, and produced in its tissue a peculiar softening and small crevices. Twice the fragments, detached from the valve by the current of blood, had produced obstructions in the capillary vessels and circumscribed foci of inflammation; in one of these cases small purulent collections were found in the muscular tissue of the heart, in the kidneys, the spleen, the liver, and both eyes; the uterus was normal. In the centre of these abscesses, as well as in a small artery, fragments of the mitral valve were found by chemical analysis. Similar anatomical changes were observed in four cases. From this circumstance, Professor Virchow draws the conclusion, that the heart is frequently the point of issue of these so-called metastatic abscesses, and that, in a certain number of cases, they are the consequence of capillary embolia, and not of pyæmia. One of the patients succumbed suddenly to softening of the heart; it is, therefore, necessary to examine this organ attentively in every case where the state of the abdomen does not account satisfactorily for the gravity of the symptoms. In a patient who had suffered with hemiplegia of the left side, the veins of the arachnoid of the right hemisphere were obliterated by clots, and a great part of the cerebral substance was œdematous, and had ecchymotic spots disseminated through it. These lesions were, however, exceptional; in most cases peritonitis was present, and was often exclusively observed. It was found in two different forms; the first was superficial, and the exudation, either plastic or purulent, was deposited on the surface of the peritoneum. In the other much severer form the subserous areolar tissue was implicated, and transformed into a detritus mixed with pus. This form is identical with the diphtheritis of the internal surface of the uterus, which most frequently occurs on the surface of insertion of the placenta.

The uterus was inflamed only in one case; the inflammation was very extensive, and had terminated in gangrene. Inflammation of the ovaries

was more frequent, and also assumed two forms analogous to those of the peritonitis. In one there existed a superficial hyperæmia, followed by abscesses in one or more follicles, and consequent effusion of pus into the peritoneum; in the other, a diffuse inflammation of the parenchyma was observed, producing considerable swelling and softening of the ovary.

Some stages of the epidemic were characterised by the frequency of phlebitis, nearly always accompanied by metastases, which Professor Virchow ascribes to embolia. The angioleucitis was, on the contrary, rarely complicated. Professor Virchow explains this difference by the arrest of the migrating particles in the lymphatic glands, where they become a source of inflammation.

The inflammation of the vessels is most frequently the consequence of ruptures of the perinæum, of the vagina, and of the neck of the uterus, and is often complicated with extensive gangrene of the areolar tissue in the true pelvis and iliac fossæ. Professor Virchow observed a case of this kind at Würzburg, in which the retro-peritoneal inflammation extended from the organs of generation upward to the diaphragm.

ART. 131.—*On the employment of Oil of Turpentine and Opium in large doses, in severe Puerperal Diseases.* By Dr. E. BONFILS.

(*Bull. Gén. de Therap.* May 30, 1858; and *Medico-Chir. Review*, Jan., 1859.)

M. Trousseau has lately employed, with considerable success, a method of treatment proposed originally by Dr. Graves, in puerperal diseases. This treatment consists in giving opium and oil of turpentine in large doses to women in child-bed who are attacked with metro-ovaritis, peritonitis, uterine phlebitis, &c. Among other cases, M. Trousseau has treated in this manner, and with success, a woman attacked with peritonitis and double pleuro-pneumonia. He also employed this plan in another case of a woman attacked with general and very severe peritonitis, which was very rapidly checked and afterwards cured; but although the cure appeared to be permanent, the patient was unfortunately seized with hectic symptoms of an insidious character, and sunk under what appeared to be a putrid infection. In the first case the opium was prescribed in pills and the turpentine in injections. At first five centigrammes (about one grain) of opium were given in five pills, to be taken daily; then the dose was gradually augmented till it reached about two grains a day. The opium was continued for thirteen days. The turpentine was administered at first in the dose of ten grammes (about two drachms and a half), in two clysters, one in the morning and the other in the evening; then the quantity was progressively augmented to thirty grammes (about seven drachms and a half). In the second case the opium was also given in pills, in the dose of five centigrammes (about one grain) for three days. The oil of turpentine was administered by the mouth in capsules, each containing one gramme (about the fourth of a drachm) of turpentine; six of these capsules were taken every day, and they were continued for six days.

ART. 132.—*The essential oils in the treatment of Puerperal Fever.*

By Mr. DOVE, of Norwich.

(British Med. Jour., April 9, 1859.)

Considering what a nauseous medicine turpentine is, that it irritates the kidneys, suffuses the eyes, and produces more or less head symptoms, Mr. Dove was induced to try, in its stead, the essential oils, selecting that of peppermint, and giving thirty or forty minims in divided doses during the twenty-four hours. He has now used this oil in seven cases of puerperal fever, and the oil of caraway in another case, with all the advantages of the turpentine. The dull colour of the complexion, œdematous condition of the surface, and offensive evacuations usually observed in puerperal fever, he thinks, point out the necessity of commencing the treatment with a stimulating aperient.

For illustration, he details the most severe of the eight cases.

CASE.—Mrs. G—, a delicate woman, æt. 20, primipara, attended by a midwife, had an easy labour, and did well for four days; but on the fifth, she complained of chills, profuse perspirations, headache, intense thirst, vomiting and purging of offensive matters, and pain and distension of the abdomen. She was restless, her countenance was anxious, breathing short and hurried, tongue covered with a white fur, pulse 160; the lochia and urine were scanty; the skin was of dull colour and œdematous. A dose composed of tincture of rhubarb and castor oil, of each half an ounce, with five minims of the oil of peppermint in a little water, was immediately administered, and thirty minims of the oil of peppermint were given in divided doses, during the twenty-four hours. A spirit lotion was applied to the head, and mustard poultices to the abdomen.

On the following day the vomiting had ceased; the headache was relieved, and the pulse was considerably reduced. The purging, thirst, and perspirations continued for a few days and gradually ceased. In this case convalescence was slow, differing from the others, in which convalescence was remarkably rapid. Instead of the loathing usually expressed where turpentine has been used, there was an evident desire to take this oil, and, indeed, to continue it, when the necessity for it had ceased.

Mr. Dove's belief is, that almost all the essential oils would do just as much good as the turpentine, and he does not think that he shall have recourse to the turpentine again.

ART. 133.—*On inflammation of the Fallopian tubes, and escape of matter into the peritoneum, as a cause of Puerperal Peritonitis.*

By Dr. A. MARTIN.

(Monatsch. f. Geburtsh., Jan., 1859; and Med.-Chir. Rev., April, 1859.)

Dr. Martin relates five cases in support of the proposition that inflammation of the Fallopian tubes and discharge of the purulent secretion into the abdominal cavity is a cause of puerperal peritonitis. He refers to a passage in Cruveilhier's 'Anatomie Pathologique,' in which that admirable pathologist suggested this explanation of some cases of peritonitis. It is right to reproduce this passage:

"The presence of pus in the Fallopian tube being an extremely frequent phenomenon in peritonitis, I have asked myself if it were not possible that peritonitis was in some cases the result of the passage of pus from the cavity of the tube into the cavity of the peritoneum; if capillary attraction or vital suction be exerted in the act of conception upon the spermatic fluid by the Fallopian tube, might not it be exerted as well upon the pus or any other liquid contained in the cavity of the uterus?"

It is desirable to give briefly the cases of Dr. Martin, so as to expose the evidence upon which his proposition is based.

CASE 1.—A primipara, æt. 22, delivered in the Jena Lying-in Hospital on the 8th of April, 1839, after a natural labour, of a strong living child. She felt a "chill" a short time afterwards. Suddenly, on the 2d of May, pain set in in the left abdomen; on the 3d of May, this was more intense, and fever was added; on the 6th, diarrhœa and delirium; on the 7th, death. The treatment consisted of twelve leeches applied on two occasions during the first days; castor-oil, opium, camphor, and ipecacuanha.

Autopsy.—A sero-purulent effusion in the lower part of the abdominal cavity; the omentum was glued to the peritoneum in front by puriform gelatinous masses. The uterus and lower intestinal convolutions were also covered with purulent exudations. The uterus was of the size of a large fist; the *left tube* was considerably enlarged in its outer third, and filled with purulent mucus; the fimbriæ were swollen. The right tube had also swollen fimbriæ, but its canal was not enlarged. The substance of the uterus was pale, normal, the inner surface reddened and covered with purple blood; there was a muco-purulent discharge at the placental seat. (It is not stated at what part of the uterus the placenta had been seated.)

CASE 2.—A primipara, æt. 33, was delivered in the Jena Lying-in Hospital on the 5th November, 1853, of a living child, after a natural labour. On the evening of the 6th, there was tenderness in the right side of the abdomen, and fever; ten leeches were applied. On the 7th, there was an offensive discharge. On the 9th, general symptoms worse; hurried breathing with bronchial secretion, diarrhœa, headache. On the 10th, death.

Autopsy.—The uterus rose above the promontory of the sacrum; its muscular structure was anæmic, containing no pus; its cavity, especially at the placental seat at the left angle, was covered with a gray-red pulpy mass, entangling shreds of vessels and plugs of blood; the cervical canal was filled with sanguinolent purulent fluid. At the abdominal extremity of both tubes was a purulent exudation, which was continued throughout the entire mucous membrane; the *right tube* was much enlarged, the mucous membrane loosened and partly covered with a yellow, purulent secretion; the right ovary was united to the posterior wall of the uterus and the rectum; the *left tube* was affected in a lesser degree.

CASE 3.—A primipara, æt. 25, was delivered in the hospital on the 12th July, 1854, after a tedious labour, of a living girl. In the night she felt suddenly severe pain in the abdomen, which increased; the belly became distended, hot, and acutely painful on moving; ten leeches applied; calomel and opium. Death early on the 14th.

Autopsy.—The abdominal cavity held a brownish flocculent pus. The lymphatic vessels of either side along the spermatic veins were much distended; the lymphatic glands enlarged; the larger lymphatic vessels contained pus and small purulent foci. The uterus was strongly contracted; muscular substance pale; vessels empty; the placental seat was on the anterior wall, two

inches above the inner os uteri; the remaining part of the inner surface was uniformly reddened, without traces of exudation or suppuration. The *tubes*, especially the *left*, were much enlarged; the vessels of the fimbriæ injected; the mucous membrane loosened, thick, and covered at the outer end with a creamy secretion. On the *left ovary* and tubes the veins were distended, and the lymphatic vessels were filled with whitish fluid.

CASE 4.—A primipara, æt. 25, was delivered in the Jena Hospital on the 19th November, 1857, of a living boy. In the afternoon of the following day, she had pain in the abdomen, and took ten grains of calomel, and on the 21st ten grains more. Several stools followed the last dose. On the 22d, the symptoms had remitted. On the 23d, however, when seemingly quite well, she got out of bed, and being surprised, leaped suddenly back again. Shortly after this, shivering and acute pain in the abdomen came on. On the 24th, the pain was especially severe in the *right side*. Twelve leeches applied. On the 25th, the abdomen was distended; everything was worse; twelve more leeches applied. Bronchitis, tympanitis appeared, and death ensued on the 27th.

Autopsy.—A large quantity of bloody purulent exudation flowed from the abdomen when opened. The omentum, where glued to the *right iliac fossa*, was inflamed. The ovaries were covered with exudation; *both tubes* at their outer extremities were much distended with purulent contents. The inner surface of uterus showed remains of endometritis.

CASE 5.—A primipara, æt. 29, was delivered in the Jena Hospital of a living boy—labour natural—on the 26th May, 1858. She was quite well for the first week, excepting that the lochial discharge was profuse and offensive. On the 3d June, the abdomen was painful, and she had three stools. During the 4th and 5th, the diarrhœa continued; she took an infusion of ipecacuanha with acetate of ammonia. On the 6th, the diarrhœa had ceased, and the patient got up to dress; while stooping for this purpose she was *suddenly seized with an acute pain* in the abdomen, which increased from hour to hour; the severest pain was in the *right side*. It was concluded that this sudden pain was caused by an escape of pus from the Fallopian tube, brought about by the sudden compression of the abdomen in stooping. Ten leeches applied. Tympanitis, delirium, and collapse followed, and death on the 10th.

Autopsy.—A considerable quantity of purulent exudation was found, *especially in the right half of the abdomen*, and the principal focus was seated in the *right iliac fossa*. There was pus in the cavity of the uterus, but no pus in the vessels or muscular wall. The *right tube* was much dilated, and contained a considerable collection of pus. The left tube was quite normal.

Dr. Martin insists upon the necessity of keeping women who exhibit any symptoms of metritis perfectly quiescent, so as to favour one of the terminations of tubal inflammation, which is closure of the fimbriated extremity. Professor Virchow, in some observations upon this paper, said it was a very difficult matter to determine the starting-point of a peritonitis. Every inflammation of the abdomen, no matter how arising, had by the law of gravity a tendency to involve the pelvic organs. Thus, in perforation of the stomach or processus vermiformis, inflammations of the tubes and ovaries occurred as much as in primitive disease of these organs. The clearing-up of this question did not rest with anatomy, but with clinical observation.

ART. 134.—*A new theory of the cause of some of the diseases of Infants and Puerperal Women.* By Mr. THOMAS BALLARD.

(*British Med. Journal*, Jan. 29, 1859.)

The proposition which Mr. Ballard endeavours to prove is—"That the instinctive act of sucking under circumstances unfavorable to the infant obtaining the food for which its system craves, is a fruitful source of disease to infants and puerperal women."

As regards infants, the exercise of fruitless and consequently prolonged and forcible sucking, is attended by an excessive reflex secretion of gastric juice, which acts upon the mucous coat of the intestinal canal, causing various degrees of injury thereto. The frequent loose and green stools of infants are the evidence of this process being in operation. A persistence of this morbid state causes many of those ailments which are usually attributed to teething, viz., erythema, eczema, convulsions, cerebral congestion, and hydrocephalus. And should the little sufferer escape death during infancy, it will afterwards be found to exhibit various forms of delicacy of constitution, attended frequently with defective appetite, as well as the imperfect development of some of the tissues of the body.

Thrush or muguet, with its attendant diarrhoea and erythema, was stated to depend entirely upon this cause. The parasitic fungus, discovered by Dr. Berg, is regarded as only an innocent accompaniment. As Rokitansky says, "it is assuredly not the morbid agent."

The ill consequences of the vain but persevering attempts to obtain its food are not limited to the infant, but are propagated to the mother, as evidenced in the excoriated nipple, the inflamed or suppurating mammae, after-pains, feverishness, milk fever, and probably some of the worst kinds of puerperal fever. In the latter case, they act as the proximate or exciting cause; the constitutional or epidemic influences being the remote or predisposing causes only.

The basis of the argument is the physiological fact announced by Dr. Brown-Séquard, in his tenth published lecture, viz., "The excitation of the nerves of taste produces an abundant reflex secretion of gastric juice, and also a flow of bile and pancreatic juice in the bowels."

The act of sucking being *par excellence* the mode of exciting the nerves of taste, it was only reasonable to conclude that if this is exercised excessively, that the secretions will be poured out in great abundance; and practical observation corroborate this, inasmuch as the frequent stools of infants, together with thrush, &c., are immediately cured by arresting the unnecessary sucking.

The facts adduced in proof of the proposition (notes of cases occurring in Mr. Ballard's own practice, carefully kept for several years), and many observations made by himself in reference to the subject, result in the following deductions.

1. There is usually little or no milk secreted by the breast until after the third day after delivery; and that the common practice of putting the child to the breast before this period, and thus inducing several

actions on its bowels, has led to the erroneous doctrine, that the colostrum, or first milk secreted, is purgative.

2. The enlargement of the breast after delivery is of the nature of an erection or hyperæmia, and is not evidence of the breast having secreted milk. Repeated attempts to obtain milk under these circumstances generally resulting in some illness, either to mother or child, or both.

3. No case of fever had occurred where the breasts had not been over-excited in the attempt either to obtain or get rid of the milk.

4. That the imperfect manner in which the act of sucking is necessarily performed by infants fed from a bottle fitted only with the skin of the calf's teat or a piece of wash-leather, thereby affording them no *point d'appui* by which they can fix the jaws and tongue in such a manner as to establish a vacuum in the mouth communicating with the cavity of the bottle, necessarily establishes that over-excitation which results in frequent stools.

5. Any other kind of fruitless sucking is attended with the same results.

6. The particular kind of farinaceous substance mixed with the milk is of little importance, the mode of its administration being the key to the food appearing to agree.

7. The solid cork-teat, fitted with a very small vent hole to a bottle otherwise air tight, seems to be the best apparatus at present in use; such is the "Bibaron Darbo," the invention of an ingenious French mechanic.

8. The index to the health of the mother and child is the state of the child's bowels; if frequent and loose, one or other is, or will be, ill; if only one or two stools a day, both are pretty sure to be well and thriving.

ART. 135.—*Spontaneous Hæmorrhage from the surface of a new-born child.*
By Dr. WM. KENT, of Nantwich.

(*Lancet*, Nov. 6, 1858.)

CASE.—On the 13th of October, I was called upon to attend a patient in her confinement, and she was shortly and safely delivered of a fine boy, which was, as usual, placed in flannel, and laid upon the bed until it could be further attended to; but as it was necessary to remove it from where it was then lying, in order to put a little more covering on the patient, the infant was removed to another part of the bed, and at that time nothing at all was noticed with regard to it; but on the child being taken up a short time afterwards, for the purpose of dressing it, it was found lying in a pool of blood. My attention was of course immediately called to it. I saw, from the position of the child, and the blood, that the hæmorrhage could not be from the umbilical cord, nor, from minute examination, could I discover that it was from the mouth, anus, or any of the usual outlets.

The child was dressed by the nurse, and she after a short time again perceived blood issuing through its clothes, when I was again sent for, the mother in the mean time being quite well.

On examining the child again, it could not at first be discovered whence the profuse hæmorrhage proceeded, but, on a further examination, I found that it was from the minute terminations of the vessels on the surface, about

and on the right hip, and with a magnifying-glass the orifices, and the blood issuing from them, could be distinctly perceived, the appearance being very similar to slightly-chapped hands or skin.

A lotion of nitrate of silver—one grain to the ounce—was applied on lint, and the hæmorrhage then commenced in the parts about the left groin, but not so copiously as before. The child, however, died from loss of blood in about eleven hours after its birth.

ART. 136.—*Iodide of Potassium as an antigalactic.* By Prof. ROUSSET.

(*Jour. de Bourdeaux*, May, 1858; and *N. American Medico-Chir. Rev.*, Nov., 1858.)

The troublesome milk-knots which tend to appear especially at the commencement of lactation, giving rise to fever, inflammation of the breast, and abscesses, indicate a diminution of the secretion of milk by therapeutic means. As the usual measures (emollient cataplasms, dieting, and laxatives) had frequently proved insufficient, the author tried the iodide of potassium. The results were as follow: The iodide of potassium occasions a considerable decrease of the milk, and in consequence prevents and removes milk-knots, particularly if at the same time the child is not put to breast. The milk returns quickly if the medicine is not used any longer than two to three days; its effect is more decided if the dose does not exceed forty to fifty centigrmm. daily. The secretion of milk can be prevented almost completely if the iodide of potassium is given on the first or second day after delivery. The author gives a full report of seven cases to confirm the above statements.

ART. 137.—*Are Babies to be taught to walk?* By ———

(*How to Manage a Baby*, a tract; and *Sanitary Review*, Jan., 1859.)

“People talk about ‘teaching babies to walk;’ but babies do not need teaching, for they will be sure to get up and walk, when their legs are strong enough, and it does them harm to do so before; in this, as in very many other things, babies would be all the better for being left to themselves. But this does not suit some mothers, who are in a hurry to see their children walk; such mothers cannot rest content without putting their children into leading strings, or go-carts, or leading them with the hand. All that they generally get for their pains is the sight of their children’s bandy legs and crooked ankles, caused by being forced to walk before their time. Who would be a baby?”

“But, though baby should not be hurried in walking, it should be allowed to keep moving all day long, while it is awake, for the limbs cannot get strong unless they are used. The best plan is, to put a piece of soft matting and a piece of carpet on the floor, and put baby down upon them to stretch, roll, and tumble about like all other young creatures. If it has a ball or a rag-doll to crawl about after, it will be ‘as happy as the days are long,’ and will, besides, be very little trouble, and be making its limbs strong, ready to walk by-and-by. It is a great pity to accustom a baby to be nursed, for it only does it harm, and gives the mother a world of trouble into the bargain. In the summer, it is a

good plan to spread the matting and carpet on the grass in the garden, and put baby down on them, to use its limbs in the pure air and light. In short, wherever it is, and whatever it does, it should keep moving all the time. The birds, the beasts, the fish, and the creeping things are scarcely ever still five minutes together in the day time. Moving brings life and health to all things, babies among the rest."

(B.) CONCERNING THE DISEASES OF WOMEN.

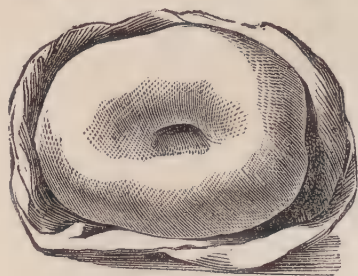
ART. 138.—*On the treatment of Mechanical Dysmenorrhœa by incising the os and cervix uteri.* By Professor SIMPSON.

(*Medical Times and Gazette*, March 12, 1859.)

For some years past Professor Simpson has thought that the best and speediest mode of cure in mechanical dysmenorrhœa, is to have recourse at once to dilatation of the os by incising it on both sides, for he found that the stricture or contraction was very apt to recur (just as so often happens with stricture of the male urethra) when the dilatation had been affected by sounds or sponge-tents.

"In order to understand the mechanism of this operation," we quote from a clinical lecture, "just consider for a moment, what is the object which you wish to attain by your treatment. A married patient applies to you for the cure of obstructive dysmenorrhœa and its usual accompaniment, sterility. Now, knowing that the one condition as well as the other is comparatively rare in the case of women who have once borne children, what you want to effect is to bring a uterus that has never contained an impregnated ovum as nearly as possible into the condition of one that has. The occurrence of pregnancy once seems to bring the uterus into a condition favorable for its recurrence, although this may not be evident in ordinary cases where pregnancy goes on to the full term, and ends in the parturition of a living child, for then lactation comes in with its counteracting tendencies. But you can satisfy yourselves of the truth of the remark as a general principle, to which there are doubtless many exceptions, by making the necessary observation in the case of mothers who do not nurse, and some of whom in consequence bear a child almost annually; and also in the case of women who are frequently aborting, for in them you will find that so soon as the immediate local and general effects of the abortion have passed away, impregnation very often occurs again immediately; whereas in the first instance it may not have supervened for some months or more after marriage. And the only appreciable difference in the state of the uterus in such a patient at the time of marriage and after parturition or after a miscarriage is, that at the latter period the os and cervix uteri are less contracted. A patulous condition of these parts seems to admit of the more ready entrance of the spermatic fluid, and so to favour impregnation. Now, then, how can we best bring the uterus of a female who has never borne children into a condition resembling the uterus of one who has aborted or borne living children? If you look

at the os tincæ of a once gravid uterus, such as I now show you, you will perceive it to be of an elongated oval form, the long axis of the opening being directed from side to side;



Sketch of the virgin os uteri.

while the orifice of a virgin uterus, such as this, is much smaller and more nearly circular. The os of the former kind of uterus is not only wider than that of the latter, but its form is different; and while, by means of bougies or sponge-tents, you may render the opening of the virgin uterus for a time sufficiently patent, you cannot by such means impart to it that longitudinal form which seems to counteract its tendency again to contract on removal of the dilating force. But all this you can effect at once, rapidly and

certainly, by making incisions of sufficient depth into both sides of the cervix uteri. To make such incisions, you require to introduce this

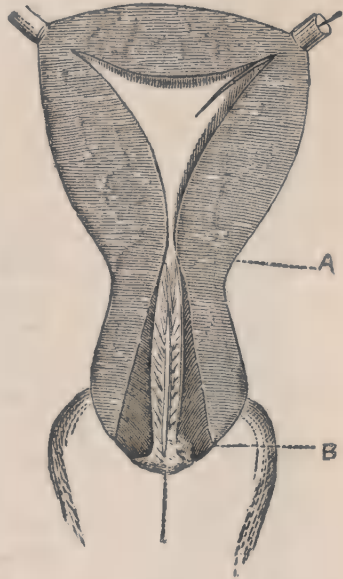


Sketch of the os uteri of a woman who has borne children.

instrument or metrotome as far as the os internum, where the incision begins—at first quite shallow, and then make it deeper as the instrument is withdrawn, till at the os externum the cervix is cut across in all its thickness. An incision of this nature into both sides of the cervix makes its canal wide and pyramidal in form, so as easily to admit the finger; and in healing leaves the orifice more like that of a uterus from which an impregnated ovum has been expelled. The first patient on whom I performed this operation, in 1843, was a lady of high rank, who had been married for several years, without having had a family, and who used to suffer at each menstrual period from most excruciating pains. She had heard about the dilatation, and had got up the whole subject—atomy and all—and came to Edinburgh with the view of obtaining relief by that means. I explained to her that the process would occupy a considerable period—two months or more, when she at once said that the time was too long, and that unless she could be cured by some speedier method she would not submit to be treated at all. I then told her that I had often thought of dividing the cervix in such

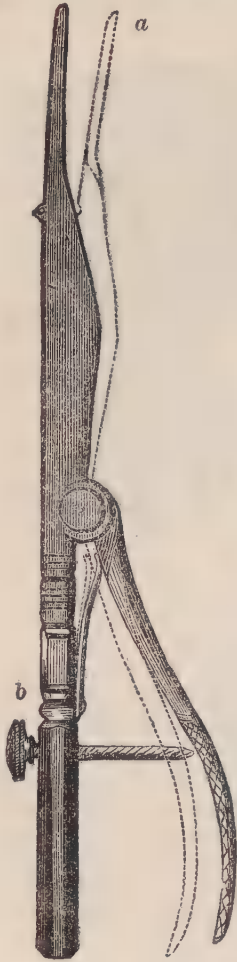
cases; and that though I had never yet put it in practice, I believed it would be both a speedy and a most effectual means of procuring relief. She readily comprehended what was meant, and seeing the feasibility of the proposal, at once said that I must perform the operation on her as the first patient. I made the incisions as I have told you, but with a very imperfect instrument, and the patient soon was perfectly well, and about four months afterwards I heard that she had become pregnant. I was afraid that the cicatrix might present some obstruction to parturition, and so was Sir Charles Locock, who was to attend her in her confinement in London. I was waiting very anxiously to know what effect the operation might have had on the labour, when a letter from Sir Charles relieved me from my anxieties, for he told me that the labour had not only gone on quite favorably, but had even been remarkably easy for a first confinement. Since that period I have performed the operation in a very great number of cases. Last week, for example, I had recourse to it in not fewer than five cases. In fact, it has come with me to be the usual mode of treatment for all cases of dysmenorrhœa depending on contraction of the os or cervix uteri.

“How are the incisions to be made? The instrument which I use for the purpose is a sort of concealed bistoury such as I now show you. The patient being placed on her left side, the point of the instrument is passed up to the os internum, and when there is a stricture at that point—which, however, is rarely the case—a slight notch is to be made there on either side. This, I say, you will rarely find it necessary to do; what you most commonly require, is to incise and open up the canal of the cervix and the external orifice. To do this you must introduce the instrument nearly to the internal os, and then, as you withdraw the instrument press out the blade and cut through the cervix on one side in such a way that at first only the internal fibres are divided, and the incision, as it becomes lower, becomes also always deeper till the point emerges somewhat below, where the mucous membrane of the vagina becomes reflected on the cervix, and below this point the portio vaginalis uteri is divided in all its thickness. You then turn round the instrument, and make a similar incision on the other side. When you have thus divided either side you will feel that a conical opening has been left, the base of which includes all the thickness of the portio vaginalis uteri. The canal may contract to some degree afterwards when the wounds heal, and to prevent this I have sometimes made use of sponge-tents or intra-uterine bougies. But the introduction of these instruments in such cases causes



Section of the uterus. The dark triangular portion on either side between A and B is intended to show the extent to which the incision should be made for the relief of obstructive dysmenorrhœa. It ought, however, to have been carried more closely to the reflexion of the roof of the vagina on both sides.

pain and irritation



The hystero-tome or metrotome. To allow of the protrusion of the blade *a*, to the desired extent, the rod *b* in the handle must be screwed out to the proper distance.

of the raw lips of the wound; and you will find that by opening up the wound every two or three days for a time with the finger, you can effectually prevent all union by the first intention, and in this way provide against the chances of a recurrence of the stricture; or you may touch the corners of the wound with a piece of nitrate of silver with a like good result. Hæmorrhage may sometimes follow division of the cervix, more particularly if you cut too deeply in the upper portion of it, where you run the risk by so doing of wounding some of the veins of the plexus uterinus; and it ought always to be guarded against by plugging the vagina immediately after the operation with some pieces of sponge. In some few instances the hæmorrhage is pretty smart; but I have never seen it occur to any very alarming extent. Inflammation may sometimes be set up and spread to the surrounding loose cellular tissue; and though this rarely goes on to any dangerous extent, yet you may expect sometimes to meet it, and you must always be prepared to treat it, and treat it according to the principles which I shall have to explain hereafter when I come to speak of pelvic cellulitis in general. Attended with such rare and slight risks, the operation is a very safe one; and there is only this further to be observed in connexion with it, that unless all the fibres are fully divided, there is sometimes a chance of the wound healing too rapidly, and the stricture being reproduced. But altogether, I believe I am entitled to say, that there are few operations in surgery so perfectly simple in their performance, and so entirely satisfactory in their results, as division of the cervix uteri in cases of obstructive dysmenorrhœa and sterility."

ART. 139.—*On diseases of the Breast.* By M. VELPEAU.

(*Gaz. Méd. de Paris*, No. 42, 1858.)

In presenting the second edition of his work on 'Diseases of the Breast' to the Académie des Sciences, M. Velpeau observed that he had met with 807 cases in private and hospital practice, in addition to the 2000 upon which the first edition was based; 200 occurring during each of the years 1854-7, with a surprising regularity. An analysis of these 807 cases (not taking into account a large number of cases which only appeared casually at the hospital consultations), shows them to consist of 407 cases of benign affections, and 400 cases of malignant disease or cancer. The 407 were thus distributed: Abscess, 116; Hypertrophy,

121; Adenoid, 130; Neurosis, 40;=407. Of the 400 cancers, the right breast was affected in 158, the left in 231, and both in 11. The ages were from 30 to 40 in 29; from 40 to 50 in 95; from 50 to 60 in 119; and from 60 to 70 in 49—the other patients being either younger or older. Among 163 cases, 60 were unmarried, 28 married and childless; 50 had borne but not suckled children, and 60 had suckled their children.

The above statistical account agrees with that already given in his first edition, in showing that the left breast is much more exposed to cancer than the right, and that it is an error to suppose cancer to be most frequent between the ages of 40 and 50, or that married women are alone liable to it. Another error is to attribute diseases of the breast to neglect of suckling; inasmuch as it is found that of 110 cases of cancer in women who had borne children, 60 occurred in those who had, and 50 in those who had not, suckled. What has been said with respect to the influence of general health, the constitution, regimen, grief, emotions, disease of the heart, &c., is equally inexact. "I have met with cancer in the robust and sanguineous temperament, as well as in the debilitated and lymphatic; in tall, strong, dry subjects, as well as in those whose tissues were fat and soft; in the gay and careless, equally as in the delicate, nervous, and sensitive. The resolute and calm are not more exempt than are the melancholy, irritable, and restless. It is met with in rich and poor, in the well-conducted and temperate, as in those who commit every excess or undergo every privation. It spares women of no country."

In respect to histological researches, M. Velpeau observes—"I have in this edition endeavoured to examine carefully what modern micrographists have taught us; for at one time I too entertained the hope that the intimate nature of cancer would be unveiled through the intervention of the microscope. This now must be acknowledged to have been an illusion. By the aid of this precious instrument there have been discovered pathological products, elements, or principles, scarcely suspected before, and their molecular composition has been far more precisely exhibited; but the malignity of cancer remains none the less a deep mystery, just as impenetrable as regards its cause and its material reason as before."

M. Velpeau believes that his work indubitably proves—1. That true, well-characterised cancer, left to itself or treated solely by pharmaceutical or hygienic appliances, is never cured and always kills—those practitioners who maintain the contrary being deceived or labouring under an illusion. 2. That we may cure radically and without relapse a certain number of true cancers by means of the knife, caustics, &c.

ART. 140.—*On the use of Iron-thread Sutures and Splints in vesico-vaginal fistula.* By Professor SIMPSON.

(*Medical Times and Gazette*, Dec. 4, 1858.)

"In the last three cases," says Professor Simpson, "I have placed and fixed around the wound, for the purpose of steadying and consolidating its walls, a slender oval splint made of the same iron wire as the sutures

are formed of. The vesico-vaginal septum is a very mobile wall or structure, with muscular tissue in it, which, in some cases, after the operation, is constantly attempting to act, as seen in the twisting, and sometimes in the expulsion of the curved catheter of Dr. Sims. Hence it has appeared to me always a matter of high moment, as regards the success of this operation, to have some means of preventing the lips of the wound being moved by these muscular contractions in the vesical walls; or, in other words, to have some means of consolidating, as it were, for the time being, the edges of the wound, and the parts situated more immediately around it. A simple stitch or suture, or a series of them, is liable to allow the wound slightly to gape under any movements in its edges. If you will try the experiment upon an opening cut in a piece of thickish leather or the like, and closed by simple stitches, you will see the truth of this remark, if you afterwards move the edges of the opening in imitation of the muscular contractions of the bladder. Dr. Bozeman's ingenious button-suture effectually prevents the disturbing effect of such movements lengthways, or in the longitudinal direction of the wound. But it has no power to prevent the evil effects of such movements if they occur crossways, or transversely to the direction of the wound. The structures on which the sides of his button or shield are placed are not restrained from movement by the presence of the button; and may (as I saw in the second case) move quite away from it, so far endangering the reopening of the lips of the wound by the mobility which is thus permitted.

"The slender, oval, iron-thread splint which I have employed in the last three cases, overcomes, as I fancy, this difficulty, as it so far consolidates the lips of the wound, and, indeed, all the parts included within the oval space, as to prevent them moving or being moved, either in a longitudinal or transverse direction. It is made by twisting ten or twenty wires of the size already indicated into an oval circlet or ring, capable of including the lips of the fistula-wound, and a few lines of the vesico-vaginal septum on either side within its concavity. It may be made, of course, and that quite readily, of any special shape or size that may be required. By a common borer, two, three, four, or more small openings can be made among the wires on each side, so as to correspond to the number of sutures used. After the edges of the wound are brought together by the adjuster of Dr. Bozeman, or any corresponding instrument, the splint is fixed by passing first the iron threads of one side of the wound, and then those of the other side, through the corresponding holes in the splint; afterwards sliding it down along the threads to its place; accurately fitting and adapting it there to the parts by the finger, a process which the flexibility of this light splint greatly facilitates, and ultimately fixing the sutures across it, tying or twisting them over the lower bar of this little apparatus. When duly adjusted and fastened, it appears to me to compress and consolidate the lips and immediate vicinity of the wound in a way which the plans previously proposed have not so completely effected. Besides, it is easily made, easily applied, and at last easily removed; for by dividing the wires below its lower bar, and turning back the splint; and then withdrawing it with dressing-forceps, the suture-wires come out along with it.

"Permit me to add, that I have found a tubular or hollow needle very

greatly to facilitate the introduction of the metallic threads. After the needle is passed through both lips of the wound, the iron wire is pushed on through the tube or perforator in it, and seized with a pair of long forceps as it protrudes from the upper end of the needle. The needle is then withdrawn and the wire left."

ART. 141.—*A Note on Ovariectomy Operations.* By Mr. ———.

(*Medical Times and Gazette*, Jan. 1, 1859.)

During the past year twelve cases, in which ovariectomy was either attempted or performed, have been put on record by London surgeons. As far as the writer is aware, they comprise all that have occurred, during that period, to the following surgeons: Mr. Brown (2), Mr. Childs (1), Mr. Erichsen (1), Mr. Hutchinson (4), Mr. Wells (4). We may, therefore, fairly group them together. Of the twelve, eight recovered and four died; seven are still living, and of these six are, as far as is known, in excellent health. The one which has ended in death, after having recovered from the operation, is a case in which the tumour was not removed. In two cases the tumour was not removed, one of the patients dying a fortnight afterwards, and the second within four months. In eight cases the end of the peduncle was brought out externally, and of these six recovered. In two, the old practice of leaving the end of the peduncle and the ligatures in the abdominal cavity, was adopted, and of these one recovered and one died. In all the cases, the tumours were polycystic, and in several they contained colloid material. In only two were no adhesions encountered, and in several very extensive and firm bands had to be broken through. When we add that the cases have not by any means been selected ones, and that in several instances the patient was in very much reduced health, it may readily be granted that this summary offers a very encouraging contrast with the results of the old method of operating.

ART. 142.—*Report on the cases of Ovariectomy that have occurred in Germany.* By Dr. AUGUSTUS SIMON.

(*Scanzoni, Beiträge zur Geburtskunde*, Bd. iii, pp. 98—122; and *Medical Times and Gazette*, Feb. 19, 1859.)

The statistics of ovariectomy hitherto published in Germany have been mixed up with English and American cases; and the object of this paper is to give an account of those operations which have been performed in Germany alone. As the operation has only become at all frequent there during the last ten years, it is still possible to obtain an exact account of the great majority of the patients, both as regards the operation itself, and the ultimate fate of the individual. To make this as complete as possible, not only has the author referred to all the published cases, but has opened up a correspondence with the surgeons of all the chief towns of Germany. In this way he has collected sixty-one cases, twenty-three of which have not hitherto been published; and these must embrace the greatest part of these which have occurred,

although it is still possible that some may have escaped notice. All foreign operations have been rigidly excluded, even when they have been performed by Germans in adjoining countries. The results of these researches are set forth in tables extending over from twenty to thirty pages. In these he gives not only the particulars of each operation as regards the mode of execution, the presence of adhesions, and the condition of the tumour; but also narrates the history of the patient both prior and subsequently to its performance, as also her later condition, whenever an account of this has been obtainable. Some of the cases, in spite of every care, remain defective in certain of these particulars.

The aim of the tables is to bring out as distinctly as possible the advantages or disadvantages of the operation; and the better to do this, the cases of ovariectomy have been arranged under three divisions, viz., 1st, those in which the operation has been followed by a radical cure; 2d, those in which death has been the result; and 3d, those cases in which, although recovery has followed the operation, the advantage has proved transitory, doubtful, or non-existent. In this last category not only have been included those operations, which begun, have had to be abandoned on account either of the firmness of adhesions or the errors of diagnosis; but others in which, although the patient has recovered from a partially or completely executed ovariectomy, yet has died eventually from after-consequences of the same, or from the effects of the original disease. Thus, a case of Martin's has been so placed, in which the patient died eight months after a successful extirpation of a colloid cystoid, owing to cancerous formations in the pancreas, lymphatic glands, and the lungs, which the operator himself regarded as connected with the original malady. So, also, with regard to a case of Küchenmeister's, wherein ovariectomy was left incomplete on account of the firmness of the adhesions, the patient recovering from the effects of the operation, but dying in three quarters of a year from the rupture of a secondary cyst. Operations like these cannot be set down as radical cures, but must be regarded as of only transitory utility, if of any utility at all, for the prolongation of life. Although these patients felt themselves entirely well for some time after the operation (in Martin's case for four months), it is very questionable whether they would not have lived longer still had mere palliative punctures been resorted to. Under the same category must in future be placed those cases in which, after complete recovery from the operation, death still eventually takes place owing to the persistence or subsequent formation of abdominal fistula; those cases in which, very soon after the extirpation of medullary sarcoma of the ovary, cancerous formations appear in other organs; and finally, not only those cases in which secondary cysts spring up after incomplete extirpation, but also those where, after the extirpation of one diseased ovary, the cysts existing simultaneously in the other forthwith increase and assume a similar threatening aspect. Each of these three primary divisions of the cases, derived from the issue of the operations, are formed into three subdivisions, dependent upon the nature of the operation and condition of the disease, viz., 1st, cases of ovariectomy completed; 2d, cases in which ovariectomy was attempted, but only incompletely performed or entirely abandoned on account of the strength of the adhesions; and 3d, cases in which, after the abdominal cavity

had been opened, the diagnosis was found to be erroneous. This last subdivision, which does not strictly belong to the statistics of ovarian tumours, has nevertheless been retained, because, in judging of the danger of an operation, errors of diagnosis frequently cannot be avoided, and their consequences must not be lost sight of. It is not indeed likely, at the present period, that Dohlhoff's error, committed in 1838, of seeking to perform ovariectomy where no tumour of the abdomen existed, will be repeated; but it is continually happening that tumours of the uterus, retro-peritoneal tumours, &c., are confounded with ovarian tumours.

It is evident that the bearing of the statistical tables must be more exact upon German practice than those hitherto published in which English and American cases have played the chief part. In these we have only the results of the practice of German surgeons (some of the most distinguished among these), whose capability of forming the diagnosis and executing the operations can be appreciated. But it is believed that these new statistics are not only of more measurable value to the German practitioner than any that have preceded them, but may also serve even in other countries as a basis for forming a judgment concerning ovariectomy. If the tables already published comprise hundreds of cases, while these only related to sixty-one cases, yet the former were open to various important sources of error, which in these it has been sought, as far as possible, to avoid. Without referring to instances in which cases have been entered both under the names of the operator and the reporter, we may advert to two sources of error which must exert a good deal of influence on the conclusions to be drawn. The chief defect is, that while cases which terminate favorably are made as public as possible, many cases having an unfortunate issue have been unrecorded. Of course it would be entirely unjustifiable for an operator to publish his successful and conceal his unsuccessful cases. But how many operators have only met with unsuccessful cases, and how many, after having published successful cases, have later met with only reverses! A great number of such cases would never be published at all; for while, on the one hand, we can scarcely blame an operator who has met with unsuccessful cases, if he wait before publishing them until he has others of a more fortunate character to place by their side; so, on the other hand, ovariectomy has become so very common an operation, especially when unsuccessful, that an operator would incur the charge of tiresomeness if he published cases of failure of the operation, unless these exhibited special circumstances of great interest. How important these considerations are, however, in weighing the conclusions to be drawn from published cases, may be judged by examining R. Lee's statistical account, published in the thirty-fourth volume of the 'Medico-Chirurgical Transactions.' With all his sources of information, he was only able to indicate 51 operators, who had performed 162 operations; but in a country where practitioners are numbered by thousands, in which ovariectomy has been recognised as a permissible operation for twenty or thirty years, and where it has become so common that Clay has performed it 69 and Bird 31 times, can we believe that this operation, which neither requires dexterity acquired only by long practice, nor complicated instrumental apparatus, has only been performed by 51

persons? This is quite incredible; for in Germany, where the operation still has many and important opponents, there have been 34 operators for 61 operations. A second important source of error, vitiating the early statistics, consists in the enumeration among the cures cases in which the operation had to be abandoned, either on account of the adhesions, or the faulty diagnosis. Now, although these women recovered, inasmuch as the disease still remained, and the patient had incurred the great danger due to gastrotomy, these cases should be regarded not as recoveries from the, but failures of the, operation. It is true that Lee has sought to avoid this error, and abstracts 60 such cases from the 162 operations; but he found it impossible to give an account of the latest condition of these cases set down as cured. These two sources of error Dr. Simon has taken every pains to avoid; and he believes his statistics claim much more confidence than Lee's, which neither comprised all the cases that had occurred, nor sufficiently controlled those they did comprise. These statistics also show that in Germany errors of diagnosis have not been of such frequent occurrence—operations not having been undertaken with the same rashness as in England and America. While in 60 of Lee's 162 cases, the operation had to be abandoned, either on account of adhesions or errors in diagnosis, this was the case in only 17 of the 61 German cases.

The following is the summary of the results, as specified in the author's detailed tables. Of the 61 patients upon whom ovariectomy was either executed or attempted, 44 ($72\frac{8}{11}$ per cent.) died from the immediate effects of the operation; in 5 ($8\frac{2}{11}$ per cent.) it proved useless, or of only transitory benefit, although the patients recovered; and only in 12 patients ($19\frac{4}{11}$ per cent.) was a radical cure obtained. (The operation may even have been more unfavorable in Germany than here represented, as notwithstanding the search made for them, other unsuccessful cases may not have been brought to light.) The operation was completely executed in 44 instances. Of these 44 there died 32 ($72\frac{8}{11}$ per cent.) from the immediate effects of the operation; 1 died eight months after in consequence of cancer in other organs; and 11 (25 per cent.) were radically cured. The operation was attempted in 15 cases, but on account of the firmness of the adhesions had to be abandoned or only partially performed. Of these, in 1 a radical cure followed; in 3 no advantage, or very transitory advantage, resulted; and in 11 death ensued as the immediate consequence of the operation. In 2 cases the operation was attempted; but the diagnosis was at fault,—one of the patients died and the other recovered.

These results are far less favorable than those which have been derived from statistics previously published; and while Fock and others place the danger of ovariectomy upon the same line as herniotomy and amputation of the thigh, it is according to the above figures more dangerous to life than the Cæsarean section, which is, according to Kaiser, attended with a mortality of 63 per cent., and, according to others, of about two thirds of those upon whom it is performed.

In a postscript, Dr. Simon brings the cases up to 64 in number. In these a radical cure was effected in 12, in 6 the operation was at most of transitory utility, and in 46 it was followed by death.

ART. 143.—*A new method of examination in suspected Cystic Disease of the Ovary.* By Dr. GRAILEY HEWITT, Physician to the British Lying-in Hospital.

(*Lancet*, March 12, 1859.)

There are many questions which the physician or surgeon anxiously puts to himself when called upon to administer relief in cases of presumed ovarian cystic disease, and more especially the question which he has to consider as to the particular method of treatment to be adopted. Thus a case of considerable enlargement of the abdomen may come before us in which the shape of the abdomen, the nature of the visceral displacement, the general symptoms and previous history of the patient, render it tolerably certain that a tumour connected with the ovaries is present. But it is extremely desirable that we should also be able to learn the exact nature of the ovarian tumour, and the presence or absence of complications, more especially if we have to decide whether ovariectomy or simply paracentesis and injection should be resorted to. Dr. Hewitt proposes, by means of an instrument which he names the "ovarian sound," to probe the interior of the cavity in which the fluid is contained. The instrument to be thus used consists of an ordinary canula and trocar; but the canula is provided with an india-rubber diaphragm having a small perforation in its centre through which the trocar passes. The sound is a slender rod, composed of the same metal as the ordinary uterine sound, and therefore, flexible, graduated in inches, and fourteen inches long. It is provided with a smoothly rounded extremity, and is fixed in a handle of convenient size. The trocar and canula having been passed through the abdominal wall, the trocar is withdrawn; the sound is then introduced through the canula before the contents of the cyst have been allowed to escape, and the operator examines the interior of the cavity leisurely and safely.

ART. 144.—*Rupture of an enormous Ovarian Cyst into the Peritoneal Cavity, and permanent cure.* By Mr. C. FARRAR.

(*British Med. Journ.*, Oct. 23, 1858.)

CASE.—"Mary B—, æt. 36, a tall, spare woman, apparently pretty healthy, about eleven years since, being at that time five months pregnant, fell over a trough on the left side, and from the injury was confined to her bed for some weeks. After her delivery it was found that the abdomen had scarcely diminished in size, and a large fluctuating tumour was detected in the left side. For several years the tumour steadily increased, until it completely distended the abdominal cavity, and acquired the magnitude which it had at the time of the occurrence of the accident.

"About eighteen months since she stumbled and fell upon the abdomen on a brick floor. She became collapsed, and had violent rigors; and in a few hours inflammatory fever set in, accompanied with accute abdominal pain. She lost flesh rapidly, was unable to lie down, and in a few days enormous anasarca of the legs and body up to the waist came on. The urine was nearly totally suppressed, and so severe were the symptoms that no hopes were entertained of her life. In about a fortnight she began to improve, and

passed daily a large quantity of turbid urine. At the same time the anasarca rapidly disappeared. One month from the date of injury the tumour had nearly disappeared.

"I examined her fourteen months after the fall. She expresses herself, with unbounded delight, as being perfectly well, and able to walk any distance, which she had not done for some years. On examining the abdomen, there are merely the hardened pedicle and sac of the former tumour to be felt, and these appear to be rapidly diminishing in size. Of course there is a large quantity of pendulous skin upon the abdomen, which time alone will reduce to its proper form and size.

"Her general health has improved very much. She has gained flesh, and has lost the wan appearance that she had previously presented."

(C) CONCERNING THE DISEASES OF CHILDREN.

ART. 145.—*Tepid injections in the treatment of Non-congenital Phymosis of Infants.* By Dr. WM. C. ROGERS.

(*Amer. Med. Monthly*, Oct., 1858.)

"I have been uniformly successful," says Dr. Rogers, "in treating the non-congenital phymosis of infants, depending upon an accumulation of the secretion of the *glandulæ tysoni* within the prepuce, by tepid injections repeated two or three times a day *pro re nata*.

"By cautiously introducing the point of a glass ear-syringe, previously oiled, within the orifice of the swollen and infiltrated prepuce, and by making gentle pressure upon the same in such a manner as to steady the instrument, and reduce still further the orifice of escape for the contained and injected fluids, I have in many instances washed out from about the glans penis a large quantity of milky secretion, in some cases entirely fluid, and in others consisting of numerous crusts or scales floating in thick puruloid mucus. The removal of the cause, the acrid secretion, has been invariably followed by a subsidence of the disease, and in no single instance have I been compelled to use the injections more than three times before the cause was entirely removed.

"I find no mention of this form of this affection in any of the standard works, and of course nothing on the subject of its treatment, and therefore lay the above before the profession, who will take it at its worth, and treat it accordingly."

ART. 146.—*On Enuresis in Children.* By Dr. HEWSON.

(*Amer. Jour. of Med. Sciences*, Oct., 1853.)

Dr. Hewson recently read a paper upon this subject to the Philadelphia College of Physicians, founded on close observation of a great many cases occurring among the children of the House of Refuge. At the time he was consulted by the managers 78 boys out of a population of 292 were the victims of the habit; but of these 15 remained but a short time under observation, leaving 63 cases, all the particulars of

which were most elaborately inquired into, and registered during the five months they remained under the author's observation and control.

The house contained a white and black division, and the affection was found to prevail in the proportion of 1 in 7 among the white, and 1 in 2·7 among the black boys—it being also most obstinate and inveterate among these last. The average age of those affected was $12\frac{1}{3}$ years, the oldest boy being 18, and the youngest 7. The general appearance of the health was good in 34; 7 boys were very strumous, and 15 were thin and palid. In 4 of the 6 worst cases the boys were stout and hearty. Of the 63 cases, 24 suffered from ascarides, and this was the case in 5 out of the 6 worst cases: in 26 there was but one alvine dejection *per diem*, and 20 boys suffered from constipation: 27 of the boys passed urine two or three times a day, as well as on rising and on going to bed; 28 passed it more frequently in the day, and four had no control over the bladder in the day. In all the cases the urine was acid, and its specific gravity was 1016 among the whites, 1020 among the blacks. The prepuce was elongated either from pulling to relieve irritation or from masturbation in 36. There were 18 who confessed to masturbation, and in 33 others there were good reasons for suspecting the practice—there being only 12 out of the 63 to whom no such suspicion attached. The influence of diet was observed by the enuresis especially occurring on certain nights; and atmospheric vicissitudes seemed to exert some influence, as a sudden fall in the barometer or thermometer during the night always seemed to increase the number of cases. Dr. Hewson publishes a curious table, exhibiting the influence of these variations during a period of 49 days.

For the treatment of the disease, the bromide of potassium was first tried in doses of $2\frac{1}{2}$ to 3 grains *ter. in die* in all the cases; and that on account of its anaphrodisiac effect, seeing that so many of the cases seemed connected with masturbation.

In nine cases of the sixty-three, a complete cure was effected within a week, but in three of these there was no reason to suspect masturbation. In four other cases material benefits resulted; but in the remaining fifty cases none whatever attended its use. The chloride of iron, together with the cold douche to the back, was next tried, but in vain; and cantharides was resorted to with as little success. Finally, to each boy who had constipation was given magnesia, and turpentine and carbonate of soda were ordered for those suffering from worms: and to all Tinct. belladonnæ was administered. A dry supper of bread alone was allowed, and the cold douche was applied. Each boy was made to rise and pass urine an hour after retiring for the night. Under this plan the number of cases diminished with astonishing rapidity.

While the affection had been so prevalent among the boys of the institution, only two of about eighty girls suffered from it, and only seldom. Their average age was somewhat greater than that of the boys; and their general health and condition far better. Their diet was the same as the boys, and their *morale* no better.

Dr. Coudie observed that the conclusions arrived at by Dr. Hewson were very similar to those which resulted from an investigation he himself had made into the subject, but under circumstances less favorable for accurate observation. Contrary to what he had anticipated, he found

that incontinence was much more common in boys than in girls ; and as far as his observation went, it was less frequent among children at home than among those congregated in public institutions, boarding schools, &c. In some cases he had found the incontinence continuing during the twenty-four hours. It occurs more commonly in children badly brought up, who had been neglected in both their hygienic and moral training. In some cases it appears to be the result of a careless, lazy, filthy habit, calling for cautious discipline ; but in the majority of cases it is the result of causes entirely beyond the control of parents or children. Dr. Condie found belladonna the best remedy ; he frequently combines it with chalybeates, as the enuresis seems frequently to be connected with a cachectic condition, or with symptoms indicative of an impoverished condition of the blood. When the incontinence occurs in the day also he has employed nux vomica or strychnia. In one point his observations differ from those of Dr. Hewson, viz., the state of the urine ; for in thirty-two cases in which this had been carefully examined, the urates and uric acid were found deficient, and there was an excess of the watery portion.

REPORTS

ON THE

PROGRESS OF THE MEDICAL SCIENCES.

January—June, 1859.

THE intention of the following Reports is to pass in review the principal additions to each department of Medical Science, which have been placed on record during the preceding six months. It is not contemplated that they should be confined exclusively to the notice of what is new; any fact or doctrine which may be considered practically useful, will, although not strictly novel, be regarded as worthy of commemoration. It must be obvious to all who are aware of the immense mass of information which is almost daily put forth by the medical press of this and other countries, that the notice of every subject would be an impossibility. It therefore devolves upon the writers of each Report, to select only such articles for retrospection as may possess superior recommendations, either of an intrinsic character, or in relation to the main end and aim of all medical knowledge—the alleviation of suffering and disease.

I.

REPORT ON PRACTICAL MEDICINE.

1. *Report of the Lancet Sanitary Commission on Diphtheria ; its History, Progress, Symptoms, and Treatment.* ('Lancet,' Jan. 15, et seq. 1859.)
2. *Observations on Diphtheritis.* By WILLOUGHBY WADE, M.B., T.C.D. 1858. Pp. 32.
3. *An Essay on the History, Pathology, and Treatment of Diphtheria.* By EDWARD COPEMAN, M.D. 1859. Pp. 47.
4. *Diphtheria ; a Lecture delivered at the Norfolk and Norwich Hospital.* By W. H. RANKING, M.D. Cantab. With a coloured engraving. 1859. Pp. 30.

At rare intervals the routine of medical practice and opinion is disturbed by the appearance of diseases, which either, like the meteor now recently departed from this hemisphere, seem to revolve in some given orbit, and to reappear after undetermined periods of time, or which break suddenly upon us with phenomena so foreign to recorded experience, as to compel us to the conclusion that we have to do with what to us is a new disease. Such was malignant cholera, and such is believed to be the disease known as diphtheria.

In pronouncing the opinion that diphtheria is a new disease, we wish it to be understood that we refer only to this country. That it has existed and been described in other countries since 1826 is undeniable, and as we shall show by reference to the able Report at the head of our list, it has even been probably known to far more remote ages. The very fact of the suddenness with which it appeared in this country, the novelty of its characters, and its alarming mortality, have not failed to gain for it a great amount of attention, and in addition to the Report alluded to, it is undergoing a formal and authorised investigation from which we are justified in anticipating the best results.

The 'Report of the Sanitary Commission,' published in the 'Lancet,' will first engage our notice. It leaves little to be desired either as regards the general history of the disease, or in the account of its progress in this country. In his introduction, the author expresses his belief, in which we entirely concur, that diphtheria "is a disease until

lately unknown to the practitioners of this country, and not formally described by our progenitors." He also notices its seeming alliances to scarlatina, but recognises at once that there are lines of demarcation as decided as those which exist between certain varieties of intestinal flux, such as diarrhœa, dysentery, and cholera. He further states, that although new to this country, it has been, as we have before said, long known in France, and that Bretonneau, who was one of its first modern illustrators, has found reason to believe that it was not unfamiliar to Aretæus. There are likewise some other authorities mentioned by the reporter, who are supposed to have described diphtheria, among whom may be mentioned Macrobius (A. D. 38), Rosen, who wrote on the occurrence of an epidemic in Sweden, and Bell, who speaks of its prevalence in America in 1813. To Bretonneau, however, we are indebted for the most succinct account of the course and symptoms of the disease, and it is from his writings that most of the more recent monographs on the malady have been compiled.

In the continuation of his historical survey of diphtheria, more especially as it declared itself in this country, the reporter remarks how impossible it is to associate its origin with any known climate or meteorological laws. As he observes, its ravages were not confined to ill-ventilated barracks and cottages, and crowded thoroughfares, but were extended over open hamlets and other well-aired localities. Vicissitudes of heat and cold, moisture and dryness, conduced no more to the explanation of its appearance, than difference in locality. Still, and with propriety, he lays considerable stress on the presence and absence of those conditions which seem favorable to the production of zymotic diseases in general, and from which diphtheria is not exempt, though it is plain that he looks to some superadded cause for its origin, the above condition being simply adjuvant to its invasion and progress.

The important question of contagion is answered by him strongly in the affirmative, and he quotes some cases which seem to prove the fact beyond dispute.

A very considerable as well as interesting portion of this Report is occupied with the history of the rise and progress of diphtheria in this country. It seems first to have been noticed in 1857, appearing in the South-Eastern counties, and described at this time by Mr. Rigden at Canterbury. During the winter of 1858 it appears to have continued with unabated severity, invading the county of Surrey, and spreading to its original seat in Kent. It also soon showed itself in Middlesex, Northamptonshire, and other of the Midland counties. In Suffolk and Norfolk it has been especially virulent, more particularly in the neighbourhoods of North Walsham, Stulham, and Aylsham.

The reporter, while he admits that these various epidemics were essentially the same in whatever district they occurred, expresses his belief that three forms of the disease might be distinguished, all however identified by one pathological phenomenon, the false membrane. Beneath this membrane the mucous tissue is generally intact, but cases are mentioned in which distinct ulceration is said to have existed. This, we may say, *en passant*, is quite opposed to our own experience, unless solid caustic had been previously applied, and to the escharotic action of which we consider it to be due. Of the microscopic characters of the mem-

brane, the reporter states, that the elements usually found are molecular particles, epithelium-scales, and pus-globules. The development of the *oidium* he justly considers to be accidental, and in no way implicated in the causation of the disease.

In his account of the treatment of diphtheria, the reporter carefully reviews the methods generally adopted, and his conclusions for the most part agree with the opinions of those who have had much experience of the disease, viz., in the condemnation of solid caustic, the advantage of less powerful local applications, and the use internally of muriated tincture of iron. The only point on which we are at issue with him is in his recommendation of tracheotomy. Neither *à priori* reasoning nor the results of experience will, in our opinion, justify us in the anticipation of any favorable results.

— Dr. Wade's brochure treats entirely of the history and symptomatology of diphtheria, the treatment being left for future publication. The work is chiefly remarkable for the mention of albuminuria, as associated with the disease, and having some specific connexion with it. The fact of the frequent occurrence of this pathological condition requires verification, but that it is occasionally met with there can be no doubt, both from the author's observations and the record of cases at this time publishing in a tabulated form in the 'British Medical Journal.' Dr. Wade's description of the disease does not differ from that of the French writers from whom he quotes, but his work may be consulted with advantage, as far as it goes, by those who are looking for a concise and well-written summary of the signs of the disease, and their pathological signification.

— The pamphlet by Dr. Copeman gives also a good account of diphtheria, as regards its history and symptoms, but it is mainly a translation of Rilliez and Barthez, several pages being occupied by an almost verbatim translation of those authors. Dr. Copeman does not appear to write from personal experience, which, as the disease has been rife in Norfolk, we should have supposed he would do.

— Dr. Ranking's brochure is founded on a lecture given at the Norfolk Hospital, and has the merit of giving a lucid description of diphtheria, from actual observation of the disease. Its value is also enhanced by a faithfully drawn and coloured engraving of the throat, exhibiting the peculiar buff-coloured membrane. The disease, as described by Dr. Ranking, differs in no respect from that of Bretonneau and other French writers. It appears in the locality mentioned by him to have assumed the most fatal characters. In his treatment Dr. Ranking condemns the solid nitrate of silver as a local application, but advises either a twenty-grain solution or a gargle of muriatic acid. Internally he has great faith in the Tinct. Ferri Sesquichloridi. He also insists strongly on the necessity of wine in large quantities, believing, and, as we think, with justice, that medicine is secondary to a vigorous support of the general power by stimulants.

On the influence of variations of Electric Tension as the remote cause of Epidemic and other Diseases. By WILLIAM CRAIG, Licentiate of the Faculty of Physicians and Surgeons, Glasgow; and Consulting-Surgeon to Ayr Fever Hospital. (8vo, London, Churchill, 1859, pp. 436.)

The influence of electricity in the causation of disease is one of the vexed questions of medicine. Latterly, it has been considered doubtful, from the manifestations of those physical agencies seemingly most active in the production of disease being interdependent, and from the difficulty, if not impossibility, of isolating the influence of any one agent from another, whether we could rightly say that this or that agent, be it heat, or electricity, or moisture, or atmospheric pressure, &c., was the *cause*, apart from other agencies manifested along with it, of disease, except in a secondary sense. Dr. Craig, however, conceives that he has just ground for believing that in the variations of electric tension will be found the remote cause of epidemic and other diseases. He rests his views chiefly upon certain physiological considerations, concerning which he writes:

“The author has long entertained the opinion that variations in electric tension on the various parts of the earth act prejudicially on those animals that may be placed on the portion of the earth which may be thus affected. In a letter which he sent to a medical friend in Glasgow, in March, 1841, there is the following observation:

“‘You will have observed ere this that my object is to attempt to discover a cause of disease different from those usually known,—a cause, moreover, which is much left in the dark, and allowed quietly to work its deadly effects, making, unthought of, its undermining devastations. The analogy that subsists between nervous influence and electricity is now recognised by physiologists. The manner in which the nerves act on the blood, in order to select the constituent of the varied complicated secretions and corporeal supporting depositions, is analogous to what is understood by chemists as electric affinity. Hence, when we consider the resemblance of this imponderable body to nervous power, working in us so wonderfully and incessantly, we cannot withhold our conviction that the great changes which occur in the amount of this electric matter, and which are affected by the operation of meteorological causes, have a mighty effect on animal life. In consequence of the heat which is generated within us so continuously, we become positively electrified, or, in other words, we have a greater amount of electricity than the inanimate surrounding objects.

“‘Well, then, on the application of moisture to the surface of our bodies, such as takes place when we get drenched with rain, or ducked in a pool of water, the good conducting power of the water in a state of evaporation withdraws the electric fluid from our system, and the nervous power is thus deprived of the amount of support which it derived from its departed electric fluid. Now, provided the amount of nervous influence, and the assistance it received from this fluid, were just what was required for the individual to guide the animal machine, and nothing more, the abstraction of electric fluid which has taken place would leave this structure unequal to its many duties.

“I am of opinion, that the source of epidemical diseases will ultimately be found to depend on the variations which take place in the state of the electric fluid. It seems to require a great effort of the nervous system to abstract from the blood the ingredients which form the complicated excretion, the urine. This seems to result from the little attraction the most of the substances composing it have for electricity, or otherwise, for what is reckoned analogous, the nervous power. Well, I would suppose that it will require a large amount of nervous power to take up from the blood the salts of the urine, and the phosphorous formations, as these have all a very feeble attraction for electric fluid, or, in other words, are bad conductors. Now, supposing the nervous power were in a weak condition, and the body in a comparatively negative state of electricity, these offensive ingredients which are contained in this excretion are not thrown off, but circulated through the system,—thence, as in cholera, the vitiation of this fluid.

“This with, perhaps, a like operation on some other of the secretions, in all probability gives rise to the tar-like appearance of the blood, and its inability to perform its important offices in the animal economy.”

“At the time at which the above remarks were written, the author had not espoused the opinion that nervous power and electricity are identical, but viewed the latter in great measure as a mere supplementary power. Having, however, since the period above stated, studied the subject more attentively, he has come to the conclusion that, if not completely, it has been all but proven, that electricity and nervous power are identical. In the following treatise he will view them in this light, as this will afford an opportunity of entertaining rational views on the very important subject of the remote cause of disease. * * *

“To be properly understood it will be necessary to carry along with us a few recollections of natural philosophy. It is a recognised fact in physics that heat and electricity are identical, and that the one is convertible into the other. It is also admitted as fact, that every atom of ponderable matter is surrounded by a little atmosphere of heat, and it is through the agency of this element that attraction and cohesion between the primary constituents of bodies are maintained. It was mentioned by Mr. William Higgins, of Dublin, as far back as 1789, that ‘all compounds are formed by the union of exceedingly minute atoms which are surrounded by atmospheres of caloric.’ It is this caloric, surrounding the atoms of simple and compound bodies, that constitutes what is called latent heat. The gaseous bodies are known to be possessed of an immense amount of latent heat. It is on the large amount of latent heat in these light bodies that their characteristic lightness depends. The vegetable bodies are almost wholly composed of these gaseous bodies, and, of course, embody less or more of the heat which is individually possessed by their constituents. The saline and earthy constituents of these vegetable bodies also contain their portion of latent electricity.”

The assumption that electricity and nervous power are identical will scarcely meet with acceptance among physiologists; and the assertion that it is a received fact that heat and electricity are identical will startle physiologists. The assumption and the assertion may be said to be, however, the cardinal points of Dr. Craig’s hypothesis; and the quotation given will show his method of physiological and physical argument.

Upon this unstable basis Dr. Craig proceeds to develop his etiological notions, and as it may reasonably be presumed that the operation of the different agencies which are active in the production of disease invariably disturb the heat-producing powers of the economy as well as the nervous force, so we shall find, upon the assumption that heat, nervous force, and electricity are identical powers, that every morbid phenomenon is accompanied by a variation of electric tension, which may be further assumed to be the remote cause of the phenomenon.

As examples of Dr. Craig's method of applying his hypothesis to disease, we may quote his remarks on "taking cold," and (in part) on cholera, simply premising that he attributes the great influence of moisture in the causation of disease mainly to the disturbance of electric tension induced by evaporation.

"— The imaginary agent most commonly accused of producing pestilential disease is *miasm*. Now, so far as I know, this miasm is not known as anything tangible,—anything appreciable by any of the senses; no search has found out its reality: yet it has been long acknowledged as an operating cause in producing epidemic disease. When we take into account the principles here advocated,—viz., that electricity and nervous force are identical, that the electricity evolved from the air in the lungs during respiration, and that separated from the ingesta during assimilation, is that which supplies the vital electricity to the nervous system, and that any cause which hinders the supply or suddenly and to a great extent withdraws it after being supplied,—there is here an appreciable combination of causes which will injuriously affect the system. 'Taking cold' will thus become an easily comprehensible idea. The escape of heat—that is, the withdrawing the electricity from the body—is understood to be 'taking cold.' The abstraction of vital electricity from a person whose nervous system has nothing to spare, will cause derangements that will be developed in some form of disease; the nervous currents in such circumstances acting on a secreting gland may be insufficient to elaborate from the blood those constituents which are required to form the various secretions; and in this manner may the secretion be imperfectly eliminated, and the depuration of the blood incompletely effected; and the retention of those elements which ought to have been given off will give rise to diseases which result from the vitiation of the fluids of the body."

"Besides the conducting power of evaporation, to draw off electricity from the earth, and the objects thereon, there may be some occult influence in operation, in the mineral strata which constitute the crust of the earth, of good conducting power, which may disturb the regularity of the distribution, and unsettle the equilibrium, of the electric fluid on the surface, withdrawing it probably into more central regions, leaving the surface in a highly negative condition, compared with that in which it was before being thus acted on. In this way may be produced those epidemic and occasional attacks of pestilential disease, which cannot be attributed even to the existence of those circumstances which are generally looked upon as remote causes. In this way may have arisen pestilential cholera, in the temperate regions of the earth,—a disease which was for a long time considered to be endemic in hot climates, as it primarily raged in those countries which are found in the torrid zone. It

was considered that, like the other pestilential diseases peculiar to hot countries, the material for its production could not exist in the colder regions of the globe. But variations of electric tension may be produced by the powerfully operating electric agencies of internal combustion and volcanic action, which must be occasionally going on in various parts of the world, and which must be felt on the surface, altering its relative electric condition. As evidence that there is more than hypothesis in this statement, the author will refer to the observations of M. Andrand, which were made during the prevalence of cholera in Paris, in the year 1849. In a communication made to the French Academy, dated July 10th, 1849, he states, that his electric machine was very powerful, and continues,—‘I have remarked that, since the invasion of cholera, I have not been able to produce on any occasion the same effect. Before the invasion of cholera, in ordinary weather, after two or three turns of the wheel, brilliant sparks of fire, of six centimetres in length, were given out. During the months of April and May, the sparks obtained by great trouble have never exceeded two or three centimetres, and their variations accorded very nearly with the variations of cholera. This was already for me a strong presumption that I was on the trace of the important fact that I was endeavouring to find. Nevertheless, I was not yet convinced; because one might attribute the fact to the moisture that was in the air, or to the irregularities of the electric machine. Thus I waited with patience the arrival of fine weather and heat to continue my observations with more certainty. At last fine weather; and to my astonishment, the machine, frequently consulted, far from showing, as it ought to have done, an augmentation of electricity, has given signs less and less sensible; to such a degree, that during the days of the 4th, 5th, and 6th of June, it was impossible to obtain anything but slight cracklings without sparks. On the 7th of June the machine remained quite dumb. This new decrease of the electric fluid has perfectly accorded with the renewed violence of the cholera, as is only too well known. For my own part, I was not more alarmed than astonished; my conviction was complete. At last, on the morning of the 8th, some feeble sparks re-appeared, and from that hour the intensity decreased. Towards evening, a storm announced at Paris that the electricity had re-entered its domain; to my eyes, it was the cholera which disappeared with the cause which produced it. The next day I continued my observations; the machine, at the least touch, rendered with facility some lively sparks.’ He states, that in the six days following the 8th of June, the mortality in Paris fell gradually from 667 to 355.”

Dr. Craig applies his hypothesis at length to yellow fever, plague, intermittent and other fevers, to pestilential diseases on board ship, and to diseases generally, and he offers one or two suggestions for prevention. His views concerning pestilential diseases on shipboard are included in the following proposition:

“That fever on board ship is caused by continuous evaporation, and consequent low state of electric tension. That on this account they ought to be kept as dry as possible, and with no water in the hold, especially in hot climates. That care ought to be exercised not to take a moist cargo on board. That in an unhealthy locality the seamen ought never to be on shore at night.”

He very properly argues that, to be successful, treatment of epidemic diseases must be commenced early; and his chief hygienic recommendations are that, those living in pestilential regions should have their habitations dry and well elevated—high situations being more favorable to health than low, from the elevated “being drier, and having less provision for evaporation;” that the beds in the same regions should be insulated by good conductors; and that the inhabitants should be out of doors as little as possible during the absence of sunshine, unless the soil be thoroughly dry. Proper nourishment and clothing are among the best methods for arresting the progress of pestilential disease—“the former to provide for the supply of nervous power, the latter to prevent its escape after being eliminated;” also, “dry and comfortable houses, and the avoidance of all kinds of dissipation and of exposure to the damp earth and air which usually prevail at times of pestilential disease.”

The Causation and Prevention of Disease. By JOHN PARKIN, M.D., late Medical Inspector of Cholera in the West Indies. (London, Churchill, 1859, 8vo, pp 191.)

There are few things better calculated to promote the well being of science than the discussion of the doubtful portions of received theories, if that discussion be carried out in a right spirit. This is an essential requisite of steady and sure advancement. Now, Dr. Parkin discusses the received notions respecting the influence of decomposing animal and vegetable matters, of the vitiation of the atmosphere from overcrowding, and of the use of impure water in the causation of disease, and he concludes from his examination of the data upon which he supposes those notions to be based, that they are erroneous, in their entirety. Dr. Parkin conceives, moreover, that he advances a sufficiency of argument to show that the effluvia from decomposing matter and a polluted atmosphere are in reality beneficial to health. Unfortunately, the author conducts his argument in so intolerant a fashion as to place it beyond the pale of criticism, and we shall simply remark that his opinion arises in great part from a misconception of the views he seeks to controvert, and from the belief that their destruction is requisite to the due maintenance of certain ideas which he entertains on the causation of disease. These ideas are worthy of note as the conceptions of a thoughtful man of world-wide experience who has clearly observed a hiatus in our present epidemiological knowledge, as well as in our knowledge of the etiology of endemic disease, and who has boldly endeavoured (although with but imperfect success) to fill up the gap. He writes—

“There is another phenomenon which the advocates of this theory [of decomposing organic matter] would do well to consider, and might be called upon to explain. This is the occurrence of disease among the inhabitants of the deep, who frequently die in large numbers during epidemic periods, as was particularly remarked during the prevalence of the Black Death of the 14th century, and also since the appearance of the epidemic cholera—a circumstance I have dwelt on more particularly in another place. This mortality occurs not merely among those small species that inhabit rivers, lakes, and ponds, but also with those ocean leviathans that are only found at a distance from land, and therefore removed from the ope-

ration of all those local causes that affect the inhabitants of the dry land. Will these theorists tell us that the decomposition of organic matter is going on beneath the waters of the ocean, and that the ferment has descended to the bottom of the mighty deep? I know not what their answer may be, but, before giving one in the affirmative, I would recommend them to study another subject; and this is the occurrence of disease in the vegetable creation,—as, for instance, among the potatoes.

“That this vegetable epidemic is due to the same cause as that which produced the epidemic cholera, it has been my object to show in a separate essay; while, also, I have attempted to demonstrate that the two diseases are the effect of *one and the selfsame poison*. Are we, therefore, to refer this disease to organic decomposition, or to another and a different cause? If the former, we should have to inquire how it happened that those products of putrefaction, some of which serve those plants for food, while the remainder prove perfectly innocuous at ordinary periods—for plants, it should be remembered, not only live but flourish in the midst of decay and putrefaction, as pigs live and thrive in the midst of filth—are suddenly converted from the elements of life and health into the elements of disease and destruction? If, on the other hand, we ascribe the disease to some other agency, the preceding theory falls at once to the ground; for if there be some general cause in operation, irrespective of organic decomposition on the surface, causing disease and death in the vegetable creation, we cannot fail to refer the production of analogous effects in man to the same agency, as it is not probable that there will be two causes in operation, at one and the same time, productive of the same or similar results. We may therefore conclude that this supplemental theory, like that applied to the production of endemic diseases, is insufficient to account for the origin of epidemics: as such we will return to the subject that more immediately concerns us, the causation of epidemics.”

Rightly considering that we possess no theory of disease which satisfactorily includes epizootics and the general diseases of plants, Dr. Parkin seeks for one, and he has come to the conclusion that the known phenomena of *malaria*, ordinarily so called, give the clue to the problem to be solved. Setting aside the opinions which have been advanced respecting the nature of malaria, he writes—

“But then the question arises, how is this invisible agent produced? If not from a cause existing on the surface, or in the soil from which it is extricated, whence can the poison be derived, and how can it be generated? It cannot be due to any cause existing in the atmosphere, being first generated in that medium, and then deposited on the surface of the earth; for the effects would then be more general, instead of being confined, as is now the case, to particular situations of defined and limited extent. Besides, there are other facts which prove that malaria cannot be generated in the atmosphere; but it is not necessary to adduce more than one: this is, that when the surface of an unhealthy district is covered with some impermeable substance, or with water, the diseases that previously prevailed will cease, and the supervention of others be prevented. The process, therefore, which produces malaria, must take place beneath, not above, the surface. Now there is only

one process that takes place beneath the surface, and which gives rise to the production of poisonous elements, independently of the decomposition of organic matter; and that process is what has been termed volcanic action. If there be no other way in which we can account for the generation of the poison, it is reasonable to inquire whether it is not to be ascribed to this particular process? This is a question that I have already answered in the affirmative, as regards epidemic diseases, and I am induced to draw the same conclusion as regards endemics. It is not my intention, however, to enter into any arguments in proof of this conclusion, on the present occasion; not only because the subject requires a separate consideration, but also because my object now is to point out what is the immediate, not the remote, cause of disease. The latter may be interesting in a scientific, but it is the former which is alone necessary in a practical, point of view. All that we require for practical purposes is to know the nature of the elements productive of disease, their situation, and the source whence they are derived. What the process is which gives them origin, and what the depth at which it takes place, are immaterial at the present moment. I may, however, be allowed to remark, it is only the hypothesis now referred to that will enable us to explain all the various anomalies which are left by the preceding theories; while all, or nearly all, the facts connected with the appearance and prevalence of epidemic and endemic diseases admit of explanation by a reference to this theory. Several facts connected with this part of the subject will be alluded to hereafter; I will therefore only mention one, which admits of explanation in no other way. This is, that those living in cellars suffer more from fever than any other class—a fact clearly established by Dr. Duncan, in his interesting ‘Essay on the Prevalence of Fever in Liverpool.’ It appears, from the statistical facts adduced by this writer, that out of every 100 dispensary cases of fever, in that town, 36·22 were living in cellars: compared with the whole population, the cellar population yielded 35 per cent. more cases of fever.

“As regards certain objections that have been made to this theory, and which will no doubt be repeated, viz., that disease does not prevail to a greater extent in the neighbourhood of volcanos than elsewhere; and that epidemics prevail in situations where neither volcanos nor earthquakes are observed; I have only to add, that these objections have been already answered in the second part of my work on the ‘Cause of Epidemics.’ These critics have not only mistaken my conclusions, but they have also confounded volcanic *action* with volcanic effects. I have never referred epidemic diseases to the eruption of volcanos, or to the occurrence of earthquakes,—but to the *cause* which produces them. These phenomena are only particular effects of a particular cause; while it is to be remembered that they only occur at long intervals and uncertain periods; but the cause which gives rise to them, and to a series of minor effects—one of which is, as I infer, disease and pestilence in the animal creation—is in constant operation. We may, therefore, have one or more of these effects, without the occurrence, or the presence of the others. But, although there be no volcano, and although no earthquakes be experienced, there may be other signs of the operation of this particular cause. There is in fact no country where

evidence is not to be obtained of the existence of volcanic action, either at the present moment, or at some antecedent period. The above objections, therefore, are rendered invalid."

A practical result which follows directly from this theory is worthy of quotation:

"Having concluded that all general and specific diseases, or, in other words, epidemics and endemics, are due to the extrication of a gaseous substance from the interior to the exterior of the earth, it follows that the best and most effectual method to prevent the injurious operation of the morbid agent would be to render this extrication impossible. Now, there is only one way in which this can be accomplished effectually, and that is by covering the surface with some impermeable substance; and this plan has been adopted to a greater or less extent, under particular circumstances, for ages. Although not employed for this special object, there can be no doubt that the paving of streets or towns has been beneficial in a sanitary point of view, while, as a matter of course, the houses themselves, when the foundation is solid, will act in the same way. When, however, there are unpaved cellars, with walls composed only of loose soil, they will produce the opposite result; for, while the extrication of the gaseous matter is not thereby prevented, its escape, on the other hand, into the surrounding air is rendered more difficult. Hence, the greater prevalence of disease among those who dwell in these subterranean abodes, as was previously shown.

"As a proof that pavement acts as a preservative, I may refer to Florence, the healthiness of which has been already mentioned; for it is principally to this circumstance that I attribute its remarkable freedom from endemic disease. Unlike other towns, which, if there be flag-stones at the side, have only loose paving-stones in the centre, while these even are frequently absent, the *whole width* of the streets in Florence is covered with a solid and firm pavement. As every large street, lane, and court, and all the principal streets, as far as the walls, are thus covered; and as the only uncovered spots are two squares and the few gardens that exist in the town, it follows that the inhabitants are thus effectually preserved from any exhalations that may arise from the surface. There is the river, it is true, but there can be few exhalations from such a stream as the Arno. In the first place, the bank on both sides, is lined with a magnificent quay; while a dam erected across the river at the lower part of the town, keeps the bed covered, the greater part of the year, with water. When exposed, however, there is no muddy bank to be seen, nothing but the sandy bottom, which the first fall of rain from the mountains hides again from view. Although I do not attribute the difference entirely to the want of pavement, for the poison is, probably, extricated more without than within the city, I may remark, that Rome is paved with small paving stones, there not being a pavement for foot-passengers, excepting in one street—the Corso. But, if the unhealthiness of Rome is to be attributed, in part, to its Campagna, and the alluvial banks of its river, so, on the other hand, it may be answered, that the absence of such a plain is the cause of the healthiness of Florence. Although the latter town is hemmed in by high hills to the north and to the south, it is open to the east and to the west, being, in fact, situated in a valley; a position that is some-

times more dangerous than the open plain, for the exhalations which arise, instead of being dispersed, may be confined by the natural walls on either side. As, however, it would not be right, in such an inquiry, to draw general deductions from isolated examples, I shall not carry this argument further, but turn to another town, London, where similar results would appear to have been produced by a similar cause.

"Although we cannot boast that the whole width of our streets is paved with flag-stones, we can yet point to the most extended and perfect system of foot-pavement that has been witnessed, either in ancient or modern times. Such an arrangement, particularly when we remember that it is extended into the majority of the courts and alleys, added to the paving of the carriage-way, which, although not so efficacious, must still be productive of some benefit, has, we may conclude, contributed to render London so remarkably healthy. This effect has, no doubt, been increased by the custom that prevails in London of having the kitchen underground; for the foundations of the houses are not only better built in consequence, but the surface beneath is hermetically sealed by the solid floor. In Continental cities, only cellars are to be found under the houses, and these in general unpaved. This, and the causes that will be presently discussed, are the only circumstances with which I am acquainted, that will account for the gradual subsidence of certain diseases, and the regular diminution in the rate of mortality, that has been observed during the last two centuries in London."

Carbonic acid gas and charcoal have, according to Dr. Parkin, the property of neutralizing malaria, and the former plays an important part in elevating the healthy status of crowded towns or dwellings, and the latter, in the form of soot, has a most beneficial influence upon the health in smoky towns. Need we say more? "True," remarks Dr. Parkin, "the amount of direct evidence is not great; but then, the indirect facts are, perhaps, of more value in such an inquiry than the direct ones. For instance, if we find that a similar result is produced by the combination of carbonaceous matter, as by the congregation of a number of individuals in a small space, and if we find, that the only agent common to each is carbonic acid, we cannot fail to refer the benefit derived to the presence of this; for it is not probable that two, or more operations, differing so greatly from each other, should produce the same result in any other way" (pp. 166-7).

In an appendix Dr. Parkin discusses the plan that is now being carried out for the better drainage of London, which plan as may be surmised from what has been said, meets with little grace at his hands.

On the treatment of Tetanus. By Mr. CAMPBELL DE MORGAN, Surgeon to the Middlesex Hospital. ('British and Foreign Medico-Chirurgical Review,' April, 1859.)

Mr. C. de Morgan's object in this paper is to recommend *a full and complete trial of aconite* or analogous remedies in the treatment of tetanus—of remedies which diminish the irritability of that part of the nervous centres which controls the reflex muscular actions, and induce

muscular paralysis, in contradistinction to those which, like opium, act on the brain and diminish sensibility rather than irritability. The treatment is to be, not by ordinary doses administered at long intervals, but by extraordinary and rapidly repeated doses. The influence of disease in controlling the action of medicines has not yet been fairly considered by practitioners, and there can be no doubt that in many instances a remedy has been set aside as useless, when in fact it had only been given in quantities which, in that special disease, were altogether insufficient. In phagedæna, for instance, as much as forty grains of opium have not unfrequently been given in the course of twenty-four hours to persons unaccustomed to its use, without the manifestation of any one symptom characteristic of the influence of the medicine upon the system. In tetanus and other painful nervous affections, a similar remark respecting opium holds good. In pericarditis, enormous quantities of calomel will be borne without the system becoming affected. Influenced by these considerations, and believing that the limit to the use of medicines in some forms of disease is not to be fixed by the boundaries within which they must be restricted in healthy states of the body, Mr. De Morgan gave aconite in a larger quantity than has perhaps ever before been tolerated. One drachm of Fleming's tincture, which is more than twice as strong as the Pharmacopœia tincture, was taken continuously for three days; and we find that in the course of ten days, three drachms of the Pharmacopœia tincture and five drachms of Fleming's tincture were taken without the manifestation of any effect upon the system, except the lowering of the pulse from 135 to 60 in the minute (the patient's natural pulse being from 75 to 80) and the abatement of the spasm; for from the time the pulse began to fall, the boy had no convulsions, and there was a progressive diminution of the chronic rigidity. Nor was this owing to any idiosyncrasy which gave the lad immunity from the effects of aconite, for when some ordinary doses of the tincture were given to him about three weeks after the subsidence of the tetanic symptoms they were found to produce their usual effects.

It will be seen that strychnia was given at one time in Mr. De Morgan's case, but it is not difficult to perceive, as Mr. De Morgan supposes, that the effects of this remedy contrast with those of the aconite in being prejudicial rather than beneficial. Croton oil was also given, but it is admitted at the end of the paper that harm and not good is done by keeping up irritation in the alimentary canal. In addition to his own case, Mr. De Morgan refers to a case of tetanus treated with aconite by Mr. Page, in the Carlisle Infirmary, in 1846—a case which we remember well, being then accidentally a *locum tenens* for the house-surgeon in that institution. He also refers to a case of tetanus treated by conium by Dr. Stewart, in the Middlesex Hospital. The cases all speak for themselves, and the only comment we will make is to hint that conium would appear to have some advantages over aconite in the treatment of spasmodic disorders, in that it is less likely to depress the already too greatly depressed circulation. We would also add that for some months we have tried conium with apparent advantage in numerous cases of epilepsy and epileptiform affections.

1. *Mr. De Morgan's case.*

"Henry Blackwin, æt. 15, employed at a coal-shed, was admitted into the Middlesex Hospital on the 16th of September, 1858, with symptoms of trismus. He is short, but well formed and strong, and has had, by all accounts, very good health.

"On the 30th of August, while walking, he trod on a large rusty nail, and the point of it pierced through the thin boot he had on, and ran into the right foot just at the base of the middle toe. There was little bleeding from the wound.

"On the following day he came to the hospital, walking on the heel of the wounded foot in consequence of the pain, and aiding himself with a stick. A poultice was ordered; a gathering formed in the part, which broke on the 4th of September, and the relief was so great that he was able to walk about. On the 7th the place was quite healed, and he discontinued his attendance. On the 8th he felt a stiffness about the jaw, which got so much worse that on the 12th he was unable to open his mouth. He felt some stiffness at the back of the neck, and pain down the back to such an extent as to interfere with his walking. He had some difficulty in swallowing. On the 13th he took to his bed. On the 15th he was unable to open his mouth or move his head, but he had no twitchings in his limbs. For two or three nights he had not had any sleep, but dozed off occasionally in the daytime. The bowels had been opened daily.

"On the morning of the 16th he was brought to the hospital; the trismus was very severe, and he was unable to move his head in any direction. He perspired freely, and complained of great pain down the back, but there was no opisthotonos. The abdominal muscles were very tense; pulse, eighty. He was ordered a castor-oil injection, and linseed-meal poultice down the back, to which was added a lotion of chloroform, aconite, and opium. A draught containing paregoric and the *Liquor Opii sedativus* was given every six hours; broth diet and strong beef tea.

"The injection only brought away a few scybala. At about ten p.m., on waking from a sleep of two hours, he had a spasm, which caused slight opisthotonos and great difficulty of breathing. The spasm lasted only a short time, and was followed by a copious perspiration. He slept afterwards for two hours.

"17th.—Feels more comfortable, is in less pain, and the expression of the face more natural. Ordered—

Olei Tiglii, ℥j statim;
Tinct. Aconiti, ℥v 3tiis horis;
Mist. Vini Gallici, ℥ij 4tis horis;
Essence of beef tea, two eggs, and milk Oj.

"18th.—Has had a fair night; but the trismus is more marked, and there is more pain down the back; the abdominal muscles are again very rigid. There is no pain in the foot.

"He was in this condition when I first saw him on my return to town. Hitherto he had been under the care of my colleague, Mr. Flower. The symptoms since the morning had been more marked, and were gaining ground. For reasons hereafter mentioned, I ordered him at once the one tenth of a grain of strychnine every two hours, the symptoms to be carefully watched, and the medicine to be omitted as soon as any effects from it were observed. The diet to remain as before.

"In the evening the muscles of the back were very rigid; he was unable

to bend his knees, and there was from time to time slight opisthotonos. He complained of twitchings in the thighs, which prevented his sleeping. These symptoms becoming more marked, the medicine was discontinued after the second dose.

"19th, 1 p.m.—The symptoms are more marked, the spasms at times being very severe, though during the night he had an hour or two of sleep. Ordered to resume the medicine.

"10 p.m.—The spasms have been increasing in severity. He has had six or seven violent paroxysms during the day, and has had continued suffering; pulse, one hundred and six; face and shoulders perspiring freely. The medicine to be taken during the night in half doses (one twentieth grain of strychnine).

"20th.—Has had no continued sleep; on dozing off has been startled by violent spasms of the hands and arms; the body rigid throughout. This morning at about five had a paroxysm so severe that he was near death from asphyxia. He has great difficulty in swallowing. The pains down the thighs and in the abdominal muscles very severe. A turpentine enema to be used, and the strychnine to be given in its former dose, one tenth of a grain.

"The enema acted well, but the spasms were increasing in frequency and violence. After the second dose the medicine was stopped; the pulse, one hundred and thirty-five; the catchings in the hand constant.

"11 p.m.—The symptoms were now becoming so urgent that the strychnine treatment could not be longer tried. It was evident that though it was producing its own specific effect, the paroxysms of the disease were in no way relieved, nor were the chronic spasms at all diminished.

"That the symptoms were due in great measure to the disease was evident from the fact that the paroxysms did not correspond in time or severity with the administration of the strychnine. The symptoms increased in severity for fourteen hours, during which no medicine was given; and at the time when the paroxysms were most severe, they became milder during periods when the medicine was still being given; whereas, when strychnine is the cause of similar symptoms, the fits correspond in time and severity with the reception of new portions of the poison. During the whole time, moreover, the chronic rigidity of the trunk and lower limbs was becoming progressively more intense.

"The strychnine, therefore, was left off, and the patient was again put upon aconite, of which five minims of the Pharmacopœia tincture were given every two hours; and as the pains in the thighs were very severe, a liniment composed of equal parts of tinct. aconite and camphor liniment was directed to be rubbed into them.

"21st.—Has had some pretty severe attacks of spasm during the night, but says he feels more comfortable; the pains down the thighs are very acute. The whole body is still quite rigid. Has taken seven doses of the tincture, but feels no effect from it. It was ordered to be continued in eight-minim doses; pulse, ninety. In the evening he complained of a feeling of soreness in the throat; otherwise, during the day, he had been freer from spasms. Ordered to take only half doses (four minims) during the night.

"22d.—Has had a somewhat better night, without any severe paroxysm; but the body is still perfectly rigid, and the pain is as severe in the thighs; less perspiration; pulse, sixty. Tinct. Aconiti, ℥viii 2dis boris.

"23d.—Remains much the same, but his appearance is better; to have a turpentine enema, and to take Fleming's tincture of aconite, ℥v every two hours, as before.

"24th.—The pain in thighs continues so severe that an enema containing m_x of Tinct. Aconiti was ordered, but it did not relieve him. In other respects he is doing well. He has lost the active spasms, but the chronic rigidity is as great; pulse, sixty; has no sensations of pricking in the hands, or any unusual feelings in the body generally. An ointment of one part of ext. belladonna and two of opium was ordered to be rubbed into the thighs, and this gave him some relief.

"From this time the improvement was progressive, the countenance becoming more natural, and the pain and anxiety diminishing. The pulse remained steadily at from sixty to sixty-five, but he had no symptom indicative of the large quantity of aconite he was taking. On the 27th the medicine was given every four hours only; on the 28th he could open the mouth a little; the aconite was given every six hours, and on the 29th three times a day. He continued to take the medicine to this extent till the 4th of October, when it was left off altogether. At this time he could feed himself and move his limbs freely; the pain had entirely left him; the abdominal muscles were still tense; the countenance natural. A little hardness about the abdomen remained for some time longer, and it was not till about the 10th that he could open his mouth freely. He was kept in the hospital till the 2d of November, and was discharged in perfect health.

"There are some points connected with the treatment in this case which seem to me worthy of special remark. The disease did not show itself in a severe form. The symptoms set in gradually, and some days elapsed, after the stiffness in the neck was first felt, before any active spasm appeared. The severity of some of the paroxysms may possibly be attributable to the strychnine. Altogether the case may perhaps be regarded as a favorable one from the beginning, though experience teaches us that even the mild and protracted cases are too frequently fatal."

2. *Mr. Page's case.*

The disease was caused by the irritation of a gun-shot wound of the forearm; and the symptoms, which increased rapidly after their first appearance, were severe. On the third day after the stiffness in the jaw had been noticed, Mr. Page began the use of Fleming's tincture of aconite, and it was continued for thirty days in greater or less quantity according to the recurrence of the symptoms, which for the last fortnight were of a very mild character, and were only severe for the first six days after the medicine was given. But it is remarkable that on all occasions the symptoms were subdued after the aconite had been fairly given. For example, on the day on which the tincture was first given "the tetanic spasms were constant and severe; the muscles of the abdomen were rigid and unyielding, and those of the inferior extremity were so stiff as to render it very difficult to flex the limbs. At noon, a turpentine injection having been first administered, three minims of the tincture of aconite (Fleming's) were given, the effect of which became very speedily manifest, and in half an hour there was an almost total remission of the muscular spasm.

"3 p.m.—The pain and spasms having again returned with increased intensity, producing a slight degree of opisthotonos, four minims of the tincture were administered, which produced the same speedy and marked effects as the former dose.

"6 p.m.—The patient has been comparatively easy since the last dose, but there is now an evident disposition to relapse. Four minims to be given immediately, and to be repeated every hour until some decided effect is

produced, the patient, of course, being carefully watched, in order that the remedial measures may be adopted should symptoms of poisoning become apparent.

"10 p.m.—Three doses have been given—in all nineteen minims between twelve and eight o'clock; at present there is a complete cessation of the pain as spasms, which, however, did not yield until after the third dose had been taken. The system is now evidently under the influence of the aconite." And so during the continuance of the disease, though at times he was alarmingly affected by the medicine, the severe tetanic symptoms were constantly subdued.

3. *Dr. Stewart's case.*

"A man was admitted into the hospital on the 7th August, 1858, with severe and frequent paroxysms of tetanus, and with permanent locked jaw. The symptoms had set in eight days before. On the 10th, he began to take Taylor's extract of conium, and continued its use in five-grain doses every two hours, and afterwards every hour, until the 26th. During this period, he took no less than two ounces and a half of the strongest form of extract, without any indication whatever of the physiological effect of conium, but with simply a gradual diminution of the tetanic symptoms. This of course was not an instance of the poison remaining unabsorbed in the stomach, as has been seen at times with opium. I should consider that here the agency of the conium was directed towards the counteraction of the morbid condition, and that hence its normal effects were not manifested."

A case of Epilepsy in which Castration was performed. By Mr. C. HOLTHOUSE, Surgeon to the Westminster Hospital, &c. ('Proceedings of the Royal Medical and Chirurgical Society,' 22d March, 1859.)

Ten cases are on record in which epilepsy is said to have been cured by castration. In the first case, the testicles were removed in consequence of disease of those organs. In the second case, one testicle was removed on account of an accident to the organ. Both patients were epileptic previous to the affection of the testicles; and their removal, though not done with the view of curing the epilepsy, did so incidentally. These cases were related to Dr. M'Kinley, of Tennessee, U.S., by a Mr. M'Gavoc, of the British Navy, and are published in the 'American Medical Gazette' of July, 1855, along with seven other cases in which castration was performed for the express purpose of curing epilepsy. Of these seven cases, two occurred in the practice of Dr. M'Kinley himself; two in the practice of Dr. White, of Tennessee; two in that of Dr. Talbot, of Missouri; and one in that of Dr. Hacker, of Louisiana. The tenth case occurred in Germany—the operation having been performed by Holz, under the direction of Joseph Frank—and it is recorded in the 'Præceps Medicæ Universæ Precepta,' vol. ii, ch. ii. Mr. Holthouse's case, as reported by Mr. Adair, the house-surgeon of the Westminster Hospital, is as follows:

CASE.—Eli B—, æt. 44, widower, native of the United States, bookseller, was admitted into Luke ward in the above hospital, on the 4th of January, under the care of Mr. Holthouse, in order to have the operation of castration performed for the cure of epilepsy.

The patient is one of fourteen children, of whom eleven are living and healthy; his father is alive, aged eighty-four, and his mother died at eighty. There is no insanity in the family, nor is any member of it afflicted with epilepsy. He was a healthy child till he was ten years of age, when he commenced to practise masturbation, and soon after had an epileptic fit, in which he bit his tongue. This was followed by severe pain in the head, and incapacity for exertion next day. The fits recurred every three or four weeks. They came on suddenly without any premonitory symptoms. During the first two years he took "skull-cap tea," without effect; his diet was also regulated. He still continued to practise self-abuse, and did not finally relinquish it till he was twenty-two, about which time he began to take nitrate of silver. For two years he tried homœopathy, the fits increasing in severity. He was at school up to the age of fifteen, when he tried a sea-voyage, but without benefit. Having returned, he sailed for South America, where he remained for two years, the fits being as frequent as before. While at New York he contracted gonorrhœa, having been accustomed to frequent sexual intercourse from the age of sixteen, in addition to the habit of self-abuse. He remained in New York for a few months, trying various remedies, amongst them sulphate of zinc, but without relief. He went again to the South for a few months, and upon his return he placed himself under the care of Dr. Kissam (his brother-in-law), who prescribed nitrate of silver, in doses of one eighth of a grain, three times daily, and in two months it was increased to half a grain. Very soon after he began to take this remedy the severity and frequency of the fits began to decrease, and he was so convinced of its efficacy, that he continued its use for about eight months, against the advice of Dr. Kissam, who feared it might affect his skin, which, indeed, it did to some extent, giving it a blue tint. At the end of this time, the fits left him for a period of two years, having gradually decreased in frequency under the use of the nitrate of silver. From the time of his contracting gonorrhœa till his marriage, he abstained altogether from sexual intercourse and the habit of self-abuse, so that during the whole time that he was taking the nitrate of silver he had no extraneous sensual excitement; yet during this period he says that he was constantly troubled with nocturnal erections, and frequent seminal emissions. Being now twenty-four years of age, he married, shortly after which he again became addicted to sexual excesses. He left his wife and his business for several months, and travelled; the fits, however, recurred every three or four weeks, and were very severe. On his return his wife died, and he remained a widower six years, abstaining altogether from sexual excesses, although frequently troubled with erections. During the six years he broke his arm, several fingers, and his leg twice, whilst in the fits. At the age of thirty he married a second time, the fits having increased in number and severity. He was often compelled to send his wife into the country for a day or two, in order to avoid sexual excitement. The fits now recurred daily. His wife died a year after marriage. After this he again abstained from sexual excesses. Dr. Horace Green, of New York, now cauterized his larynx daily with nitrate of silver, and at the end of three or four months he would be free from fits for nineteen days; when they did recur, they were so slight that he scarcely lost consciousness, and did not fall down. This plan of treatment was pursued for two or three years, at the end of which time he became attached to another young woman, which revived all his old amatory feelings, and the fits began to increase in frequency, recurring at intervals of fourteen days, when they would continue daily for a week, and then cease for fourteen days more. Galvanism was now tried with some slight beneficial effect. Next arsenic, in

the form of Fowler's solution, which he continued till the fits recurred daily, and he became so prostrate that he was confined to his bed. For a long time he took iron to neutralize the effects of the arsenic, but for months he was compelled to walk on crutches. He came to England two years ago to have tracheotomy performed by Dr. Marshall Hall, who had advised it when he saw the man in America. Dr. Hall died soon after the man's arrival, and he went to Paris, and was under the care of M. Nélaton. Afterwards he placed himself under M. Trousseau, who gave him belladonna, which affected his vision but not his fits. Dr. Delasiauve next treated him with camphor for four months without effect. He returned to England, and was under Mr. Simon, at St. Thomas's Hospital, in order to have castration performed, in which he had great faith, for he attributed his fits chiefly to sexual excitement, which still troubled him much; but his wish was not acceded to. He took bromide of potassium without any benefit, and then the nitrate of silver for two or three months, in half-grain doses three times a day. The skin became darker than before, and the fits recurred daily. He next went to Germany, and was there sounded for a stone in the bladder on account of frequent micturition, which he has had since infancy. No calculus was present. He was an inmate of the hospitals of Vienna, Prague, and Dresden. He left the latter in October, 1858, and was admitted into the Westminster Hospital, under Dr. Radcliffe, on the 30th of the month, and remained in two months, during which period he took quinine and iron, and camphor, but without avail. Since his second wife's death he has entirely abstained from sexual intercourse, though he has been constantly troubled with nocturnal erections, and occasional seminal emissions, and these continued up to the time when he came under the care of Mr. Holthouse, to whom he applied to perform castration, which after much deliberation he consented to do; and it was performed upon both testicles on the 4th of January, 1859, under the influence of chloroform. Two or three hours afterwards there was considerable hæmorrhage, which was checked by the application of cold. He had one fit during the hæmorrhage. His face has a bluish-slate tinge, which pervades the body, but the colour is darkest on the face. His fits are of the rotatory kind, preceded by a sudden scream, and lasting not more than a minute, and when over he is quite himself again. In the fit which he had while in bed after the operation, he did not scream, but merely struggled violently.

January 5th.—He had another fit this morning.

6th.—The fit recurred early this morning.

7th.—At four this morning another fit occurred. He says that after his second marriage the fits frequently followed immediately on the act of connexion.

8th.—Has had no fit at all to-day.

9th.—Had a very slight attack this morning, scarcely more than a giddiness for a minute. Altogether, since the operation, the fits have been exceedingly mild.

Mr. Holthouse brought this case before the Royal Medical and Chirurgical Society, partly because the result is not so favorable as in the cases referred to, and partly for the purpose of answering the objections which it had provoked. We leave Mr. Holthouse to speak for himself, merely adding that the patient had been previously in St. Thomas's Hospital, that a consultation was there held upon him, and that Mr. Simon would have castrated him if the other surgeons of the hospital had given their consent to the operation. The reasons which induced Mr. Holthouse to operate are thus stated by him:

"1. The patient's urgent request. 2. The simple and dangerless character of the operation. 3. The knowledge that epilepsy had been cured by castration. 4. The possibility, if not probability, that it might be in the present case. Lastly, I was further influenced by a consideration of the history of the case, which showed, first, that every remedy hitherto tried had failed; and, secondly, that there was a close connexion between the origin and severity of the fits and the condition of the sexual organs. Whatever weight may be attached to the 2d, 3d, and 4th reasons, I confess I should not have felt myself justified in recommending a proceeding of which I had no personal experience, nor any knowledge of the individuals who had adopted it; but although these considerations would not have sufficed to make me recommend the operation, they had their influence in inducing me to consent to its performance. It will be obvious, then, that I was chiefly influenced by the wishes of the patient. The question, therefore, arises, how far may a patient's wishes as respects treatment be acceded to, and on what points must a physician or surgeon satisfy himself before acting on a patient's suggestion? The following seem to me to be essential:—1st, he must satisfy himself that the patient is of mature age; 2dly, that he is of sound mind, and not the subject of hallucinations or a monomaniac; 3dly, that his proposition is not unreasonable; and lastly, that the remedy proposed is not a dangerous one. Now, the only points on which I apprehend there can be any difference of opinion are—1st, as to the sanity of the patient; and 2dly, as to the reasonableness of the treatment adopted. I shall, therefore, address myself to these two points. And first, as to the sanity of the patient, it has been asserted by many that the mere fact of the man having had epilepsy for so long a period was presumptive evidence that his mind was unsound, and the pertinacity with which he begged to be castrated was considered a sufficient proof that this opinion was not unfounded. Mania, I was reminded, sometimes takes this form, and lunatics have been known to castrate themselves. Now, I am free to admit that if such a desire existed, or such a request were made on the part of any individual without adequate motive, there would be legitimate grounds for believing that he was insane, or at least, labouring under an insane delusion. But was there no adequate motive in the present case? What are the facts? The patient is walking in one of the streets of New York, when he is stopped by a physician of eminence and repute, the editor of a medical journal, who informs him he has just received a communication from one of his correspondents, in which castration is recommended for certain forms of epilepsy attended with great venereal excitement, and that nine cases had been successfully treated by this method. The letter containing this announcement Dr. Reese reads to him, and advises him to submit to the operation, at the same time offering him an introduction to Professor Parker, surgeon of the Bellevue Hospital, and professor of surgery at the University of New York, and one of the most eminent surgeons in that city. A consultation is held between Dr. Reese and Professor Parker, when the latter, having been made acquainted with the history of the case, and had his attention called to the cases published in Dr. Reese's journal, consents to perform it, and is only deterred from doing so by the interposition of a medical relative of the patient,

who was of opinion that no benefit would result. From this period is to be dated the desire of the patient to be castrated; and believing that he had at last found a remedy for his disease, is it to be wondered at that he should be earnest and urgent in his desire to avail himself of it?—that he should prefer the positive evidence of those who had tried the remedy, to the negative evidence of those who had not—the practical experience of the former to the theoretical considerations of the latter? Moreover, when he called to mind the origin of his fits—their recurrence on his first marriage, after they had been absent for the two preceding years—their exacerbation on his second marriage, as well as under any sexual excitement—and his frequent nocturnal pollutions—could he dissociate the fits from the sexual organs? And was not the fact of his thus associating them a proof rather of his sanity than of his insanity? I appeal to the candour of the society whether there is the least analogy between a man desiring castration under such circumstances, and the morbid craving for mutilation of a madman? I would further remark, that the removal of the exciting cause of epilepsy, as indeed of any other disease, has been long recognised as the established rule of practice; and, although it is not always possible to determine what this cause may be, I hold it to be generally admitted that we are justified in acting on a fair presumption of the cause, though such presumption must necessarily fall short of actual proof. Acting on this presumption, epileptic patients have been subjected to the dangerous operation of trephining, sometimes with and sometimes without success. Acting on such presumption, Wardrop cured a case of eccentric epilepsy, by removing a joint of a healthy finger. Tissot relates a case where a similar good result followed amputation of the great toe. Dr. W. H. Edwards, of Virginia, U.S., cites another, in which the leg was amputated a few inches below the knee-joint; and several similar or analogous examples are on record. It must be obvious, then, that in all of these cases the epilepsy was of eccentric origin, and that the great nervous centres were only affected secondarily and sympathetically. Now the whole history of the patient whose case forms the subject of this paper, pointed to the abuse of the reproductive organs as the original source and the subsequent exciter of the epileptic paroxysms; indeed, the early abuse to which they had been subjected had so increased their functional activity that they were habitually in a state of abnormal excitement, and this reacting on the brain prevented that repose of the organ so favorable, if not essential, to its recovery, and constantly tended to counteract the effect of other remedial agents. But I am assuming here that the brain is affected; and indeed, in so long standing a case, it would be absurd to suppose that this organ was not in some measure damaged, although it may not have been originally the starting-point of the disease. I am aware it may be objected that the excitement of the genital organs was the effect and not the cause of disease of the brain, and that the origin both of the fits and of the sexual excitement must be referred to cerebral disorder. Now, though it may not be possible to prove the negative of this, there are so many facts which demonstrate the influence of the reproductive organs over the cerebral functions, that it is no unfair inference to suppose they may have thus acted in the present case. I hold, then, that the removal of all extrinsic sources of excitement from a dis-

ordered brain is both reasonable and proper; and believing, as from the history of the case I was entitled to do, that the reproductive organs of this patient did exercise an injurious influence on his brain—seeing, moreover, that precedents were not wanting where the removal of the testicles, under like circumstances, had been attended with success—I say, taking into consideration all these circumstances, I maintain that the request of the patient was not unreasonable, and that it was perfectly justifiable to accede to his wishes. The results of the operation are, so far as can be judged of at present, certainly not such as the patient anticipated: the fits continue to recur with much the same frequency, and are of a similar character.”

A case of Topical Medication of the Larynx. By Drs. HORACE GREEN, BEALES, and MOTT. ('Amer. Med. Monthly,' Feb., 1859.)

The excitement connected with this case, not only in New York, but throughout the length and breadth of America, has not yet had time to die out. At first, a sweeping condemnation was passed upon Dr. Green, and the death of the patient was unhesitatingly laid at his door; and even now the professional and public mind is only half pacified by the resolution of the Academy—a resolution come to after a discussion carried on through three several sittings—“that we, the Academy of Medicine, after a full examination of the reports of the case, and the post-mortem examination, do consider that the death of Mr. Whitney was in no wise the consequence of improper treatment, but was the unavoidable result of a complication of disorders.” At any rate, Dr. Green has a right to complain of the want of proper professional feeling in one or more of those who had to do with the case after he had done with it. For ourselves, we do not venture to express an opinion; and we content ourselves with simply giving the data upon which an opinion may be formed, viz., the account of the case by Dr. Green, and the subsequent account by Dr. Beales and Dr. Valentine Mott.

CASE.—1. *Dr. Horace Green's account of the case while under his treatment.*

“On the 25th of October, 1858, Mr. S. S. Whitney (a son of one of the wealthiest citizens in New York) called upon me, and requested to place himself under my care, for medical treatment. His health, as he stated, had been bad during several years, and for a twelvemonth past he had more or less cough, which cough had increased considerably of late. It was quite severe by spells, he said, and was attended with slight hæmorrhage, which, *he believed*, came from his throat. His face was pale and thin, and his general appearance was indicative of a phthisical condition of the system. I examined his chest by auscultation, in the presence of Dr. Richards, who, as is usual in the cases I examine, made a note at the time of the physical signs observed. I take from this record: chest thin; a little depression is observed on the left thoracic wall, with less expansion on this side; percussion gives a flat sound over all the upper portion of the left lung; slightly dull on the left side. On applying the ear to the chest, a distinct *humid râle* or ‘click’ was heard below the left clavicle, in both inspiration and expiration—which, when accompanying the above signs, is positively indicative, in

my experience, of the presence of tubercular softening. His throat appeared granulated and inflamed; the left tonsil was slightly enlarged and ulcerated; the epiglottis was thickened, and its border whitened with a line of erosions.

"The enlarged and ulcerated portion of the left tonsil was removed, the pharynx, the sub-tonsillary fossæ, and the border of the eroded epiglottis were cauterized. An alterative mixture was advised night and morning.

"This alterative was continued by Mr. Whitney during the three following weeks.

"October 26th.—Applications of a solution of nitrate of silver were again made to the fossæ, epiglottis, and into the glottis.

"27th.—The same treatment continued. After this, I saw nothing more of Mr. Whitney until the 9th of November, when he returned and requested to have the treatment continued. At this visit, and again on the 18th, cauterizations of the glottis and larynx were employed. I now spoke to him of the necessity of giving more attention to his case if he expected to be benefited by the treatment. He named some cause for his long absence, and promised to be regular in his calls thereafter. The soreness of his throat had disappeared, and his cough was less for a time, but latterly it had increased again. He expressed much anxiety about his lungs, and at this visit it was proposed to employ the tube and injections into the left bronchus, as soon as the parts were prepared for this operation. For this purpose the topical applications were continued to the opening of the glottis, and into the larynx. It was, I believe, at a subsequent visit, on the 20th, that I made another careful examination of his lungs. Unequivocal signs of a cavity in the superior portion of the left lung were now observed; for, in addition to *humid râles* in this location, the respiratory sound was distinctly cavernous.

"December 4th.—Another interruption of two weeks occurred, when Mr. Whitney returned, and the sponge probang was again passed into the larynx.

"6th.—It had been my intention for several weeks to employ tubage of the larynx in this case, as soon as the normal sensibility at the opening of the glottis was sufficiently overcome to allow the introduction of the instrument. But the patient's visits had occurred at such long intervals that I found the parts were not properly prepared. But as Mr. Whitney had several times expressed a desire to have it used, I resolved on the 6th to make the attempt. The tube was therefore introduced, and a drachm of the nitrate of silver solution, of the strength of fifteen grains to the ounce, was injected into the left bronchus. No irritation whatever followed this operation. The patient's next call was on the 9th of December. At this visit he expressed much satisfaction with the effects of the injection, stating that his cough and expectoration were both diminished, and he desired that the injection should be again employed; but, for reasons hereafter named, only the glottis and larynx were cauterized, as in previous operations; and the patient engaged to call in two days and have the tubage repeated, but he failed to meet this engagement, and did not return until the 14th—five days afterwards. This was the last visit Mr. Whitney made at my office; and as most unjust and utterly unfounded reports have been made and widely circulated with regard to the character of this operation and its effects, I shall describe briefly, but minutely and exactly, the steps of the operation. After much experience in catheterism of the larynx, it has been fully ascertained that this operation can be performed with greater certainty, if employed soon, or within one or two days after the opening of the glottis has been cauterized. Hence these applications are usually made once or twice between each operation of tubage.

"When Mr. Whitney came to my office on the 14th, Dr. M. E. Foy, a member

of this Academy, was present. He had expressed a desire to see the operation of tubage employed on some of my patients, and when Mr. Whitney came and took his seat, I remarked to Dr. Foy that it had been my intention to employ the tube for him on that day; but as he had not had an application to the glottic opening for five or six days, I was fearful for that reason of not succeeding, and as I had other patients on whom the tube operation was practised, I should not use it in Mr. Whitney's case, but employ the sponge probang. *The sponge probang was employed, but the tube was not used that day on Mr. Whitney.* It was never used but once in his case, and that was on the 6th of December, eight days before this last operation. The same probang, the identical instrument which on some ten different occasions previously had been passed into the glottis and larynx of the patient, was employed, and in precisely the same way, except when the sponge reached the glottic opening the patient partially closed the throat (a thing occurring every day with nervous or sensitive patients, and, as every operator knows, without the occurrence of any harm to the patient), by which the progress of the instrument was suddenly arrested, so that it did not enter the windpipe at all. It was at once removed, no more force having been used than that which is constantly employed every day in operations on the air-passages. The operation was not renewed, and the patient, after talking a while with Dr. Foy and myself, and remarking that 'the operation hurt him more,' or that 'he felt it more than usual' (which arose, as I said to him, from the sudden arrestment of the instrument), he left, with the arrangement that he should return the next day and have the tube employed. These are the precise steps, and the particulars of the last operation. Dr. Foy stood directly by the side of Mr. Whitney, and saw every part of the operation, and can testify, I doubt not, to the entire correctness of this statement."

Dr. Foy corroborates these statements in every particular.

2. *Dr. Beales's account of the case after the cessation of Dr. Horace Green's attendance.*

"December 14th, 1858.—About one in the afternoon I was called to see Samuel S. Whitney; I found him surrounded by several members of his family, in a state of the most intense excitement, suffering, and terror; in answer to my inquiries as to what had happened, he answered—'Sit down, Beales, and I will tell you the truth; I was such a fool as to go to Dr. Green to be operated upon, and the d—d villain has killed me.' His countenance was pale and haggard, and had all the appearance of a man whose nervous system had received a severe shock; his breathing was occasionally irregular, and almost spasmodic, coughing almost incessantly, and speaking with great difficulty and pain, in a hoarse and unnatural tone of voice; his skin was cold and clammy, and covered with perspiration; the pulse was extremely frequent, feeble, irregular, and intermittent; he was excessively restless, not remaining in the same place more than a few minutes at a time; complaining of intense pain in the region of the larynx, shooting through to the cervical vertebræ, and down the course of the trachea to the chest; he kept grasping the larynx, and reiterating every few minutes that he was murdered; I endeavoured to calm the excitement of the patient, and tried to examine his fauces and throat, which appeared in a state of great inflammation; I discovered no lesion, as, in fact, on account of the pain and terror of the patient, the examination was necessarily very imperfect, as he would scarcely allow the spoon to touch his tongue, and I concluded, therefore, to defer the examination till he should become more quiet. I gradually ascertained, partly from the family and

partly from himself, that he had been several times to see Dr. Green; on the first occasion his tonsils had been amputated; on a subsequent occasion, ten or twelve days previously (the exact dates were not told to the relator), 'a hollow tube had been passed into his lungs, and about a teaspoonful of solution of nitrate of silver had been injected into them by touching a spring at the top of the tube.' Whether this was done more than once the relator does not recollect to have been stated. On the 14th of December Mr. Whitney breakfasted with his family, appearing to be in his usual health; he afterwards went to Dr. Green's office; 'the doctor passed an instrument into his throat, and finding some obstruction, he pushed the instrument with some force; he (Mr. W.) felt something give way, immediately experienced severe pain about the top of the windpipe, and told the doctor he had hurt him;' he returned home, informed the family of what had occurred, and I was called as before stated. 1 p.m., I saw him with the symptoms and in the state previously described; it was evident that, under these circumstances, the only indications that could be followed were to rally the patient's strength, to produce some reaction, and to moderate the local irritation in the fauces.

"7 p.m.—Is suffering severe pain, described to be in the larynx, down the course of the trachea to the chest, and round to the cervical vertebræ; pulse 112, feeble and irregular; still excessively restless; other symptoms are about the same; insisted on my remaining with him all night.

"15th, 3 a.m.—They called me, as they observed the face to be swelling; I found extensive emphysema all round the neck, and partially in the face, rather more noticeable on the left side; he had continued exceedingly restless, scarcely dozing for a few minutes, breathing very irregular; pulse 106; urine scanty, very high coloured, and turbid. Continued the same remedies and nourishment.

"1 p.m.—Heat of surface more natural; scarcely any pain in the chest, emphysema very much increased round the throat and face, and extending down the chest; has not slept; has taken scarcely any nourishment, on account of the pain in swallowing; could not continue the inhalations, although they rather relieved him temporarily.

"8 p.m.—Dr. Valentine Mott saw him, in consultation with me. Is decidedly worse; emphysema very much increased; neck and face enormously swollen, it has extended all over the chest, but lower down on the right side; breathing somewhat laboured; pulse very feeble, irregular, and 112; skin is again covered with clammy perspiration, and about the neck and chest of a purplish erysipelatous appearance; does not particularly complain of pain, except on talking or swallowing.

"16th.—Upon the whole has passed a more comfortable night; symptoms are all a shade better; the emphysema rather less in the face, but the throat and the chest are enormous, the mammæ resembling those of a stout nursing woman.

"17th.—With Dr. Mott. Has slept more during the night, sometimes for nearly an hour at a time; has taken more nourishment, but there begins to be considerable mucous secretion, which interrupts his respiration, and gives him great trouble to expectorate; pulse very irregular and feeble; the slightest movement increases its frequency; it averages about 108.

"18th.—With Dr. Mott. Has passed the best night since the attack; there is a decided improvement in all his symptoms; emphysema slightly subsiding; pulse 90; is rather more hopeful.

"19th.—With Dr. Mott. Passed a very bad night, principally owing

to the great increase of the mucous secretion, that keeps him almost constantly coughing and expectorating, which he does with great difficulty and suffering; the pulse very frequent, feeble, and excessively irregular: although it is certain that there is some serious lesion in the vicinity of the glottis, yet it is utterly impossible to ascertain the state of the parts; the emphysema has rather subsided about the upper part of the face, so that he can partially open his eyes.

"20th.—With Dr. Mott. Has passed a very bad night; breathing laboured, and all the difficulties of swallowing, &c., increasing; the emphysema rapidly disappearing from the face and throat; abdomen distended and tympanitic.

"21st.—During the night he became rapidly worse; did not swallow after 2 a.m., and died rather suddenly at 8 a.m., partly from exhaustion, and partly from asphyxia."

3. *The post-mortem examination.*

This was made by Dr. Alexander B. Mott, in the presence of Dr. Valentine Mott and Dr. Beales, and the record is signed by all three. Dr. Horace Green was not present, nor any of his friends.

Dec. 22d, 1858.—"Thirty hours after death nothing peculiar in the appearance of the body. Rigor mortis quite moderate. On making an incision from under the chin, in the mesial line of the sternum, it was remarked that the anterior projection of the thyroid cartilage was more than ordinary. Directly as the knife divided the deep cervical fascia on the left side of the thyroid cartilage, pus issued out; a little further division opened into a cavity, containing pus, about the size of a large hen's egg, and extending a little in front of the pharynx, and downward behind and below the thyroid cartilage. At the upper and posterior part of this abscess there was an opening into the pharynx, large enough to admit the end of the forefinger. This abscess was lined by a large quantity of destroyed filamentous tissue, hanging from different parts of it like wetted tow. The entrance into the œsophagus immediately below this was perfectly sound, internally and externally. The larynx was now laid open from behind, and at the first glimpse a red point about the size and shape of a grain of wheat, on the left side, a little below the left chorda vocalis, and running longitudinally, led us to exclaim, there is the point of laceration of the mucous membrane, by which the air has escaped into the cellular tissue to constitute the emphysema. On close inspection, and wiping the part with a sponge, no abrasion or aperture could be discovered. Every other part of the larynx and trachea, as far as removed, presented on its internal surface a perfectly normal appearance. Indeed, we all remarked, that we had never seen a larynx and trachea more natural and healthy. We next concluded to have a look at the bronchi and lungs. Perhaps about an inch above the division of the trachea, the most beautiful vermilion redness that we ever saw on a mucous surface commenced and extended into each bronchus, but greatest in the left, and extended down each lung. Over this peculiar redness there was a cloudy shade, which vanished after a short exposure to the air. On opening the pleura, the upper lobe of the left side, at first glance, seemed covered with white thick pus. But, on close examination, it proved to be soft, strumous-like fibrin, easily rubbed off. This, on the side and posterior part, connected that lobe in patches to the pleura costalis. These imperfect adhesions were easily broken down with the fingers. The whole of the upper part of this lobe was very red and solid—hepatized. Just at the root, or at the commencement of the bronchial rami-

fications, there was an open cavity, about the size of a small black walnut, of a reddish-brown colour, and irregular villous surface, as though a slough had separated. At the upper and anterior part of this cavity there was a small opening through both pleuræ. This lobe was cut into in different directions, but no tubercles could be found. The lower lobe was perfectly healthy. The redness of the mucous membrane of the right bronchus extends to the lung of that side, but the three lobes were perfectly normal. There were no old adhesions on either side of the cavity of the chest. Some little appearance of the emphysema remained."

An experimental Inquiry on Endocarditis by the synthetical method. By Dr. RICHARDSON, Lecturer on Physiology in the Grosvenor Place School of Medicine, &c. ('British and Foreign Medico-Chir. Rev.,' January, 1859.)

The special object of this very important paper is to show that endocarditis can be artificially produced in an inferior animal. Of sixteen experiments, in which an ounce of a solution containing ten per cent. of lactic acid was injected into the peritoneal cavity, not one failed to give during life and after death all the required phenomena. The proof is positive and complete.

These experiments have also a bearing of great interest in relation to the physiology of the sounds of the heart. For the sake of connecting pathology with symptoms, Dr. Richardson divides the symptoms, keeping purely to those which are endocardial, into four stages—(a.) The stage when the action of the heart is simply excited, *the sounds being normal*. At this time all the valves are free, and not thickened, but the endocardium is of a brilliant vermilion colour. (b.) The stage when the *first sound is lost and the second accented*. In this the auriculo-ventricular valves are thickened and œdematous, and lying so close to each other, that, when the heart was contracting, they must have cushioned against each other, and prevented regurgitation *passively, i. e.* without tension or movement. (c.) The stage in which the first sound is replaced by a faint purr. In this the auriculo-ventricular valves are beginning to lose the thickened and œdematous condition just mentioned. (d.) The stage when there is marked and sustained systolic bruit. In this stage the auriculo-ventricular valves are restored still more to their normal condition. In all these cases the semilunar valves of either side are scarcely affected. Now in every instance when the first sound was lost, an inspection of the heart showed clearly that the auriculo-ventricular valves were so modified that their tension was clearly impossible; and in every instance where the systolic murmur was established, the same valves were implicated, but in a different way, *i. e.* they were partly restored to their action, but indurated in structure. In every case the second sound was distinct, and in no case were the semilunar valves so implicated as to interfere seriously with their action.

"Better," says Dr. Richardson, "than any physiological or physical inquiry, these results prove to my mind that the systolic sound is due to tension of the auriculo-ventricular valves, and the second sound to tension of the semilunars. What can be more conclusive? Two animals are well—they have two sounds to their hearts, and two sets of valves

in alternate play; the animals are made ill by experiment, and the first of these sounds is entirely lost; you listen to the heart for hours, and there is the one solitary tick: it is listening to a clock; you destroy, and cut down to the heart of one of these animals in this stage, and you find the ventricular valves inactive; you let the second animal have longer respite, and the first and lost sound returns, but modified as a murmur. You kill and cut down to the heart of the second animal, and you find the ventricular valve in a position to act, it is true, but devoid of flexibility, incapable of tension, and as it were fitted up for the production of murmur by the blood-stream."

Returning to the cases of induced endocarditis, it is seen that they differ from cases of spontaneous endocarditis *in the disease being primarily manifested on the right side of the heart*. This is a remarkable difference, for in spontaneous endocarditis the disease is almost entirely confined to the left side of the heart; but Dr. Richardson is not without an explanation—an explanation, moreover, which opens out other fields of thought. He says—

"The cause of the difference in the two classes of cases, I mean the induced and the spontaneous, seems then to be simply this. In the case of induced endocarditis, the poison introduced into the body by an absorbing surface finds its way into the circulation by the venous blood. It follows that, as the poison traverses the circulatory canals, it comes in contact with the inner surface of the right side of the heart first: in the pulmonic circuit it undergoes some loss, and so entering the left cavity is less active in its effects. In other words, the poison in these instances, in so far as the heart is concerned, is derived from the systemic circuit, and is lost in part in the pulmonic circuit.

"On the other hand, in rheumatic endocarditis the evidence all points to the supposition that the poison is a product of respiration. Hence, as the poison traverses the circulatory canals, it comes in contact, first, with the inner surface of the left side of the heart; while, in the systemic circuit, it undergoes loss or combination, so that the blood returning by the veins is not poisoned, and the right side of the heart escapes.

"Reversing the previous proposition, the poison in these cases, in so far as the heart is concerned, has a pulmonic origin, and a systemic destruction.

"The further inference from this argument also is, that the action of the producing poison, both in the artificial and the spontaneous endocarditis, is directly on the part affected, *i. e.*, by contact with the endocardial surface. Let us examine this question from another point of view.

"Seeing that a certain series of changes are produced in the endocardial membrane when the necessary condition, *viz.*, a producing poison, is present, our reason admits of but two modes by which the changes induced could originate. Either the poison has been carried into the affected part through the nutritive vessel or vessels of the part, and thus has produced its effect, *a tergo*, by interference with nutrition; or it has been applied to the free surface of the part, and has produced its effect by direct contact, like a blister applied to the

skin. Many poisons have the privilege of producing their effects by both methods; but in reference to lactic acid and endocarditis, and in reference to an assumed rheumatic poison and endocarditis, there is no alternative but to accept that the action of the poison is by direct contact with the free surface.

"For the position of the question is this: that in the artificial endocarditis the right side of the heart is first affected; in the rheumatic endocarditis the left side is primarily, and by a general rule which has but rare exceptions, singly affected. Now, it is clear that, if the effect of the poison in these cases, one or other, were *a tergo*, *i. e.*, by introduction to the endocardial surface through the nutritive vessel, the two sides of the heart would share equally in the catastrophe, inasmuch as they are both fed from a common source and the same blood. But if the action of the poison is by direct contact of the poison with the free surface of the membrane, the occurrence of endocarditis in the induced cases on the right side, and in the spontaneous cases on the left side, is easily and satisfactorily accounted for.

"The observations here made lead me to recur for a moment to a remark already offered relative to the methods which have been pursued by those chemists who have searched for an acid condition of blood in rheumatic cases. As far as I can discover, every inquiry in this direction has been made on venous blood. But it is obvious, if the foregoing arguments are correct, that the venous blood is never charged with the free poison. The poison being disposed of in the systemic circuit by elimination and combination can only be present in *arterial* blood, whither the chemist should turn for evidence."

Nor have we yet exhausted the subject of this important paper. Thus, among others, Dr. Richardson argues, from this production of endocarditis by lactic acid, "that a special poison, present in the blood and bathing all tissues, may have its influence as a disease-producing agent, localized in one structure or organ." He shows also that the ordinary sequences of the inflammatory process are the same in endocarditis as in other inflammatory disorders, and that (contrary to what he once supposed) the beads which fringe the margins of the valves originate in exudation beneath the endocardial surface, and not in simple deposits from the blood. He does not deny, however, the possibility that the beads thus formed may be the bases of a secondary deposit from the blood. He shows, also, that the tendency to fibrinous deposition in rheumatism cannot be due to the neutralizing of the true blood solvent by lactic acid (as some have supposed), by showing that lactic acid is itself a fixed solvent of the blood.

On Diseases of the Stomach, with an Introduction on its Anatomy and Physiology. By WM. BRINTON, M.D., F.R.C.P., Lecturer on Physiology and Forensic Medicine in St. Thomas's Hospital, Physician to the Royal Free Hospital. (Post 8vo, Churchill, pp. 406, 1859.)

The neat portable volume we have before us claims to be the product of fifteen years of clinical observation and research, directed towards a

group of diseases, in which the labours of Abercrombie and Cruveilhier had still left a vast, and almost unexplored, field for succeeding inquirers. Hence, with rather more of deliberation and finish than we often find in medical literature, and with materials the wide and elaborate character of which have been favorably noticed by some of our critical contemporaries years ago, it offers us much that is novel, in the sense of being hitherto unpublished, and much more that is likely to be incidentally new to many of our readers, from its having been hitherto only included in papers and essays little accessible to the profession at large. It is to these points that we purpose limiting our attention.

A preliminary chapter, which gives us the author's views on the anatomy and physiology of the organ, and purports to be derived from his Physiological Lectures, may be regarded as an abridgment of his article "Stomach," in the 'Cyclopædia of Anatomy.' It is illustrated by woodcuts drawn by the author. It contains the following points worthy of notice.

The movements of the stomach are examined from the observations of Beaumont on the one hand, and the vivisections and experiments of the author on the other. It is thus deduced that, with variations mainly determined by the characters of the food, and the stage of digestion, the gastric movement is a peristalsis, impressing on the gastric contents a slow uniform circulation; forwards at the surface of the organ towards the pylorus, backwards on its centre towards the cardia. The mixture and comminution of the pulpy food with the gastric juice are thus materially assisted.

To the pylorus, again, the author assigns a very different function from that attributed to it by previous writers.

"The structure of the pylorus already described, and the movements of the stomach just specified, demand a very different view of its action than that selective power usually attributed to it, and implied by its very name (*πυλωρὸς*, *portæ custos*). Far from being a specific and independent structure, which contracts against the food in the earlier stage of digestion, but subsequently relaxes to permit the passage of the chyme, it must be regarded as a mere terminal thickening of the transverse coat; with a strength proportional to its bulk, and an office, not only closely analogous to that of the transverse fibres, but almost identical in both stages of gastric digestion. Instead of relaxing only at the end of this act, to allow a moderate peristalsis to urge through its aperture a selected portion of food, the pylorus is, at all periods of stomach-digestion, a contracted inflection of the transverse coat; through which the more fluid and homogeneous parts of the gastric contents are continually being strained, in small quantities, and at frequent intervals, by a more or less violent muscular effort—by a process, in short, of coarse filtration, aided by mechanical pressure."

In describing the anatomy of the mucous membrane the author confirms (we use the term without implying any priority) Koelliker in some points; but extends the description of this excellent histologist, as to the minute central cytoblasts of the tube lining the gland-cells in the dog, to the human subject, in whom he further points out some peculiarities of the oval epithelia themselves. A careful summary of the chemical characters of the gastric juice follows. Following out the

strictly inductive path the author prescribes to himself, the contrasts of the gastric juice with the *liquor sanguinis* suggests to him an hypothesis (or rather summary) of its origin; and leads to a description of its properties, and an inquiry as to the nature of its action.

"In answer to this question we may premise, that it is obviously no simple process of solution by a dilute acid; no mere catalytic influence (like that of spongy platinum in the acetification of alcohol;) no mere fermentation (like that excited by yeast in a solution of sugar); no mere complex acid combining with protein-compounds as bases (as in the 'hydrochloro-pepsic' view propounded by Schmidt).

"If we must connect the above details by some theory, we may first remark, that the gastric juice dissolves protein-compounds; that it renders them highly soluble; and that it assimilates their form and reactions to its own, without changing their composition. For any parallel to such a process we can only look to those lower degrees of chemical action, where solution and combination, adhesion and affinity, may be supposed to meet and merge into each other; where proportions are tolerably definite, but true equivalents indistinct; and where, though form is changed and reactions modified, elementary composition remains little affected. Actions of such a kind may be found in the union of many substances with water, or its elements, to form the compounds called hydrates. And the conversion of protein into peptone, by the gastric juice, presents so many analogies to the formation of a hydrate, that it seems not impossible the chief office of this secretion may be, that of enabling water to combine with the various members of the albuminous groups of alimentary substances; in order to their acquiring that solubility, and uniformity of constitution, which must probably precede their admission into the current of the blood."

"It is not impossible that the acid commences the process by a slight, though genuine, solution of the more resisting substances. And at any rate, this constituent seems to have the power of checking putrefaction, if not of arresting all metamorphosis, in the other ingredients of the secretion; like the small quantity of oil of vitriol which is added by the chemist to hydrocyanic acid with the same object."

The nature of the secretory process is only so far gone into, as to call forth a denial that the gastric juice consists of expelled cell-growth; and a conjecture, that the dimorphous cells—columnar and oval—prepare the acid and organic principle of this secretion respectively.

Lecture I considers the symptoms of gastric disease generally. After a brief introduction, levelled point-blank at his lecture-room audience, the author treats successively of pain, vomiting, hæmorrhage, flatulence. The first he refers to the physiological relations of the stomach; and discusses its origin, locality, character, and distinctness; and the aid afforded by pressure in inquiring into its causes. Vomiting he defines as chiefly and essentially an act of the abdominal muscles: a view in which, five years ago, the author's Essay in the 'Cyclopædia of Anatomy' was in complete opposition to the existing opinions and statements on this controverted subject.

"There is ample evidence that the act of vomiting is effected mainly by this abdominal pressure; which is not only indispensable to it, but (as proved by vivisections) suffices to effect it when reduced to a contraction

of the diaphragm, or of the abdominal muscles, or even to a slight muscular compression of the hypochondria.

"The exact aid given by the contraction of the stomach is less capable of determination. That any such assistance not only can be, but often is, altogether dispensed with, it is scarcely possible to doubt. But, on the other hand, it seems equally certain that the abdominal pressure, to which the act of vomiting is chiefly attributable, is often accompanied, and assisted, by a contraction of the muscular wall of the stomach itself. And as might be expected, observation on man and animals during life shows that this contraction specially engages, not only the pyloric valve, but the neighbouring muscular pyloric extremity of the organ; in movements which are probably rhythmic (contraction regularly alternating with relaxation), and peristaltic, and which there is certainly no sufficient ground for supposing to be ever anti-peristaltic."

Hæmorrhage is chiefly considered as regards its semeiotic bearing; variable, but small. Flatulence, however, again takes us to Physiology; and brings our author forward with a deliberate and explicit contradiction of the power hitherto assigned to the alimentary canal, of secreting gases. Ingestion of air, decomposition of ingesta, and less frequently, of the secretions of this tube also, are the only three sources of such gases admitted by the author. The evidence on which he bases this novel view is too long for extraction, but deserves a careful consideration. The view itself leads him to conclude that—

"In respect to flatulence, as a symptom of gastric disease, we may start with these propositions. That the stomach and intestines generally contain a certain quantity of aeriform fluids, derived, in great part, from the decomposition of ingesta. That it is only where they are excessive and troublesome that their presence is strictly abnormal. And that, among the causes of such an abnormal amount of these gases—in one word, of flatulence—the most immediate and obvious are (1st) a quantity of food which is too large, either absolutely, or relatively to the digestive juices of the individual; and (2d) a quality of food which (either from existing or nascent putrefaction, or from a peculiar proneness to it, or even from a peculiar composition) favours this change."

Some important hints as to the relation of pain and flatulence follow the above deduction, and end this chapter.

Lecture II illustrates the circumstances connected with the examination of the stomach after death, and the subject of gastritis.

We quite share the author's opinion as to the importance of a full study of the processes of death and putrefaction; and almost regret his reserve as to "the errors (both in pathology and anatomy) to which" the delicacy, and the mechanical arrangements of the gastric structures, specially give rise. However, Goethe's rule—never to oppose error by any more direct denial than the dissemination of truth—applies even more strictly to professional than to scientific men.

Engorgement, hypostasis, shape and size, mammillation, effusion, gastric solution, putrefaction in the stomach—these are the changes successively studied under the above head. If confirmed, the author's detailed and connected statements bid fair to modify the whole morbid anatomy of the stomach.

"In like manner I must so far anticipate some unpublished researches

as to point out, that there are various causes which can respectively oppose and further the solvent effect of the gastric structures, by introducing modifications precisely corresponding in their results to those which we may observe in experiments on artificial digestion. Especially would I notice, that the protection of the tissues of the stomach against its own solvent juice seems to be effected by the secretions—chiefly salivary and biliary—it receives from the neighbouring mouth, œsophagus, and duodenum; and that it certainly secretes no alkaline mucus (I would almost say no mucus whatever) in a healthy state. Hence among local causes capable of preventing gastric solution, we may notice: (1), the presence of alkaline saliva and bile in any quantity; and (2), the presence of animal food, absorbing and engaging (in proportion to its minute division or surface) the powers of the pepsinous fluids furnished by the organ. And conversely, among influences capable of increasing the solution produced by a given amount of healthy gastric substance, we may mention (what has often concurred in the cases recorded) a vegetable or starchy character of food: (1st), as offering little substance on which the stomach can immediately expend itself; and (2dly), as producing by its decomposition, aided by that of the organ and its secretory contents, an amount of acid such as adds the very ingredient necessary for an energetic gastric solution, to a pepsinous fluid itself remarkably deficient therein.

“With all this, however, it must be confessed, that the more energetic and complete cases of self-digestion of the stomach can scarcely be explained, except by supposing a true secretion of gastric juice; by a process which, so far as we know, invariably precedes death, and which is almost always connected with its normal immediate cause; namely, the presence of food in the stomach. Whether injuries of the head, or of any parts of the nervous centres, can provoke an increased secretion of gastric juice, or can evoke it independently of the presence of food in the organ—or, if so, how far such a result would be comparable to Bernard’s well-known experiment, in which an injury of the medulla oblongata excites an excessive formation of grape sugar by the liver—are questions to which our existing information affords no answer.”

Gastritis, rarely if ever present as an acute mucous inflammation, is viewed as a *nexus* of symptoms, discernible in poisoning, and witnessed chiefly in its subacute and chronic varieties. This immunity of the stomach, from acute idiopathic inflammation, is then inquired into; and contrasted with the frequency and curability of the more chronic varieties, which, in dyspepsia, seem almost of a healthy—at any rate a conservative—import, in their influence on the organism generally.

“Whether any part of this immunity is specifically gastric, and is connected with the solvent action of the stomach on the protein-compounds, I hardly dare to inquire. But we may bear in mind the density of the matrix in which the cylindrical stomach-tubes are packed side by side; and the repletion of these tubes with a two-fold cell-growth, such as allows no exsudation from the surrounding capillaries to reach their minute calibre, save through a thick mass of pepsinous cells, and a thinner but more solid layer of smaller cells. And while, on such a structure I would found a plausible conjecture (remember, a conjecture only) why inflammatory exsudation rarely involves the whole mucous

membrane, I cannot forbear adding, that careful researches entitle me to conclude that, in all the more sub-acute varieties of gastritis it is the free mucous surface—the inter-tubular ridges, the stratum of columnar epithelium belonging to them and to the open mouths of the tubes, and the subjacent vessels—which the inflammatory process chiefly, if not exclusively, affects.”

In the remarks on catarrh, hæmorrhagic erosion, and follicular ulceration, the author is again original to the very verge of heterodoxy, if not beyond it; throwing great doubts (so far as we understand his guarded language) on the share of the stomach in secreting the ropy mucus from which morbid anatomists have named the first, and on the really follicular nature of the last disorder; and deranging our notions of hæmorrhagic erosion, as a very frequent and slight lesion, by questioning whether it may not be imitated by congestion and solution of the organ after death. What is to become of the statistics of hundreds of semi-putrid stomachs, collected by some diligent observers, irrespectively of gastric symptoms during life, it is almost painful to inquire. But Dr. Brinton's views await confirmation from others. If they are thus shown to be true, we shall be almost thankful to have been led back from the mere dead-house to the bedside again. Unenlivened by clinical medicine, the niceties of putrefaction can scarcely have any permanent interest save for the worms, or our old friends of the Arabian Nights, the ghouls.

Lecture III, on ulcer of the stomach, is derived from the author's elaborate monograph on this disease, fully noticed in a preceding volume of the 'Abstract.' A few additions refer chiefly to ætiology and treatment.

Lecture IV, on gastric cancer, again, is partly based on the copious materials used in the author's Essays on the Pathology and Symptoms of this malady, in the 'British and Foreign Medico-Chirurgical Review' of a few months back. We can recommend this and the preceding chapter as containing a fuller and more exact account of these, the two chief structural diseases of the stomach, than any work hitherto published. The treatment of cancer leads to the statement (by implication) that the disease is often latent, to be roused by specific causes, and is rarely relieved by remedies.

“Looking back at my own experience, I must say that I have done more good by careful feeding than by any drugs in the Pharmacopœia. These cases, however, have been chiefly among hospital patients. In other words, they were instances in which the charity of the unprofessional public made me the means of suspending that destitution and wretchedness by which cancer can be produced, and aggravated, in the strictest pathological sense of these words. Pray acquit me of exaggeration if I say that, more than once, as certainly as I have seen vaccination reproduce cow-pox, I have witnessed misery, anxiety, and starvation, suddenly rouse into fatal activity a gastric cancer: and that, this cancer having run its course in a few weeks, the necropsy has verified anatomical details, conclusively proving that it must have been present as an unsuspected tumour for months, or even years, during which the patient has literally enjoyed the most robust health. And even in a case of this kind, I have known agony and prostration converted into comparative

ease and cheerfulness during the patient's last few weeks of life, by the comforts of an hospital, among which I especially distinguish light, air, warmth, stimulants, and food of proper quantity and quality."

Lecture V concerns maladies some of which are so rare, that we may not invidiously distinguish it as chiefly pathological.

"The maladies to which it refers, though exceptional in one sense, are not so in another. For even the rarest among them are not (at least in my judgment) the mere extremes or modifications of a variety of diseases: but rather constitute types and classes for themselves. And not only are they of great pathological interest, but their collected cases are quite numerous enough to afford deductions of much practical importance in the treatment of those more common gastric maladies which are allied to them. I may add, that my remarks are all founded on clinical and pathological observations; and that I hope they throw some new light upon the maladies of which they treat;—maladies, some of which are not only obscure in their nature (and therefore little likely to reveal themselves to the casual or one-sided glimpses which their infrequency has caused to be bestowed upon them), but even obscured by the names and descriptions they have hitherto received."

Cirrhotic inflammation, or *plastic linitis*, is described by Dr. Brinton in two forms: one affecting the whole organ, another (and far commoner) chiefly or exclusively engaging the pylorus. Of the first he says—

"But what is the pathology of the lesion? Regarding it as (what it obviously is) both inflammatory and gastric, shall we therefore name it *gastritis*; adding (it may be suggested) the term *interstitial*, to distinguish it from inflammation of the mucous membrane? Or looking to the evidently exalted nutrition and vigour of the muscle, and the thickening of the areolar tissue by a structure somewhat analogous to itself, shall we therefore call it *hypertrophy*? Or shall we look to its hardness to supply us with a name—*sclerosis*? Or find a more characteristic title in the histology of and arrangement of the deposit—*fibroid infiltration*?

"Now, though names are even more important in pathology than in some other sciences (simply because they exercise even more than their customary influence on ideas), it would scarcely be right successively to question the applicability of each of these terms to this lesion, unless for the sake of the inquiry, as well as of its results. But to call such a lesion 'gastritis' is almost as objectionable as it would be to call pleurisy, pneumonitis. For the latter termination of the name of an organ ought to be reserved for inflammation more or less specially involving its peculiar structure—that structure which secretes gastric juice in the stomach, and interchanges gases with the air in the lung. While the involvement of the gastric mucous membrane in this disease is inconstant, late, and incidental. Then, again, 'hypertrophy' is clearly a misnomer, both as regards the whole organ, and the areolar tissue; portions of the original mass of which may often be found atrophied and decayed in the new deposit that entangles and surrounds them. While, even in respect to the muscular coat, we have seen that its genuine hypertrophy is not an element of the disease, but an attempt of Nature (so to speak) to remedy or palliate the effects of the lesion: an attempt which appears to be sometimes wanting, and generally temporary.

'Sclerosis' we need not criticise: since, apart from its applying chiefly to the latter stages of the lesion, its etymology affords no sufficient distinction, either from scirrhous, or from calcified deposits. 'Fibroid infiltration' is less objectionable than any of the preceding. But the adjective overstrains an analogy: the substantive misstates a fact. For the histology and progress of the deposit is very unlike fibroid. While not only do we never see it as an infiltration, but what we do see by no means looks like the solidification of the liquid deposit this word ought to connote: inasmuch as it differentiates the various tissues, instead of soaking into all in tolerably equal amount."

"Its other and closer analogue is found in cirrhosis of the liver. In this disease we may trace so close a resemblance to the lesion under discussion, and a resemblance which extends to so many circumstances of the deposit—its nature, its situation, its relation to the vessels, its connexion with the portal system, its contraction, its effects on the adjacent original structures—that really the application of the term *cirrhosis of the stomach* seems by far the best means of connecting its gastric analogue with our existing nosology."

"Although any new name is likely to be merely provisional to future pathological discoveries, I would suggest that the inflammation of the filamentous network of areolar tissue ensheathing the vessels, which seems the main characteristic of both lesions, might be well expressed by some such word as *linitis* (from the Homeric *λίνον*, *rete ex lino factum*)."

Of the second, which many excellent pathologists still regard as cancer, and which we believe Dr. Brinton was the first specifically to distinguish from it, he states—

"In general, the decision turns upon facts by no means difficult to detect or interpret. When we find a lesion, which has been perceptibly present for several years, and in spite of this duration, and of a considerable bulk, has yet failed to fuse the coats of the stomach together; which leaves intact the muscular (if not the mucous) tissue; which extends laterally, but not vertically, in and between these coats, so as to thicken a large extent of stomach without forming any tumour; which tends to condensation and contraction, rather than to circumferential or interstitial deposit, and in contracting, shrinks into a quasi-cartilaginous mass, but rarely calcifies, and never softens; which only causes ulceration of the mucous membrane very late in the malady; and lastly, which contains scarcely more cell-growth than the cicatrix of a gastric ulcer, and is not associated with the presence of cancerous deposits in other organs;—when many (if not all) of these characters are simultaneously present, there need be little hesitation in refusing, to such a lesion, the name of cancer."

After the symptoms and treatment of this malady a suppurative form is also described: and tumours, atrophy, hypertrophy, are successively considered, before dwelling on the extremely interesting subject of dilatation; the several varieties of which state suggest some novel hypotheses as to its essentially nervous origin. A similarly recondite cause is also assigned to a curious (and hitherto unnoticed) double inflammation of the intestinal tube, in which the stomach is usually one of the segments affected.

The last lecture, on dyspepsia, is the least complete (but, to our mind, the most interesting, not to say amusing) of the whole series; and quite explains why Dr. Brinton chose this form of publishing his views. Nowhere but in a lecture-room, and to an attentive and sympathising class, could any one dare to give so flowing and bold an outline of a difficult (almost a dangerous) subject. The subject, the lecturer, the student, the profession, the outlying quacks, the public at large, are all noticed in turn, wherever they suggest anything of interest to the audience. It is, indeed, chiefly as a critical and suggestive summary of dyspepsia that the author sketches the subject. How far dyspepsia is gastric; how far it is structural; why its symptoms refer to the stomach, and occur in so many diseases;—and especially its subdivisions according to its symptoms, the date of digestion, the kind of food resented, the gastric fluids expelled (as in pyrosis)—are questions which are briefly considered, without any concealment of the existing deficiencies of our knowledge. The causes, nature, and treatment of the disorder close this chapter, of which we may give the following samples.

Speaking of medicines and diet—

“If any of you should (not unnaturally) wonder at my bringing into an invidious contrast, measures which it is the duty of the physician to combine in the treatment of any given case, let me explain that I do so from a motive for which I hope your sympathy. This particular congeries of maladies (the pathology of which we have found is still, from various causes, less precisely and accurately established than that of most other diseases of the alimentary canal) has for centuries been the subject of more quackery than perhaps any other of the ills that flesh is heir to. Not to dwell upon that liability to differences of opinion which the votaries of medicine proverbially share with those of other arts and sciences, and which cannot but be prominent in such a subject as the therapeutical action of drugs—not to trace (what, I think, the most charitable observer of human conduct could scarcely fail to recognise) the germs of quackery in some of the specifics against indigestion which have emanated from the ranks of our own profession, I will only remind you of the systematic impostures which live, and thrive, in great part on the treatment of dyspepsia. As a malady which is often slight, almost always easily curable; which peculiarly affects the affluent and luxurious; and is itself sometimes the result of a form of sensuality which perhaps implies, and certainly produces, a somewhat enfeebled state of the mind—dyspepsia is, from all these reasons, the vantage ground of that numerous (but illegitimate) offspring of Esculapius which is sustained by the trumpet instead of the caduceus. Dyspepsia is, indeed, the malady above all others, in which a variety of quackeries work their pretended cures. It is the efficacy of diet and regimen that explains the small nucleus of facts around which they cluster their mendacious statements. And the very same considerations which thus expose systematic imposture, are those which we must recognise, if we would avoid casual error; if we would know what can be effected by drugs in the treatment of dyspepsia, and which drugs we ought to select. With some doubt as to whether those I recommend to your notice will be found by others to possess all the merits which experience leads me to assign them, and with a full

certainty that we have much to learn respecting them, I may at least point out with what cautions they are to be appraised.

"A person suffering from dyspepsia takes a certain remedy. He recovers. Therefore he is cured by it. So runs the popular conclusion. But the conscientious physician can accept no such flattering or delusive estimate of his remedy. And if the power of the latter be specifically the subject of inquiry, he asks himself, 'Was the recovery due to that spontaneous amendment which sometimes occurs in this disease, as in most other diseases: or was it due to the diet and regimen adopted simultaneously with the drug? Are its traditional virtues, again, in any degree confirmed by its physiological effects on the healthy subject? And, as regards its therapeutical influence, can it be traced apart from diet? Can it be verified as a matter of frequent experience, and (in the case of the stomach) as an immediate result of its ingestion? Lastly, can it be confirmed by the results of interrupting, omitting, and repeating its administration?' Judged of thus strictly, our remedies would doubtless be fewer in number: and discoveries of new specifics less frequent. But the art of medicine would gain by the extension of its scientific basis. Quackery would lose its chief attractions and excuses, by the exposure of this fundamental delusion of the public as to the cure of disease. The public might become healthier, as well as wiser, by learning that the means of health lie rather in the circumstances with which we constantly surround ourselves, than in rare and exotic drugs. And lastly, our practice would gain far more than an equivalent for a mere number of remedies in the increased skill and efficiency which would result from concentrating our study on the best way of wielding a few of tried and unquestionable virtues.

"Such a scrutiny of our Pharmacopœia would no way affect its value, save to place it, if possible, on a still more irrefragable basis. Nor do those occasional results of the *vis medicatrix Naturæ* (aided perhaps by diet and regimen) which are vaunted as cures by the homœopath, or which may be deduced from the far rarer (but more trustworthy) observations of our own profession on the natural history of disease, by any means disprove the value of drugs as the armament of the practical physician. He has not to answer the abstract question—whether Nature and diet can accomplish the cure of disease—but to discharge the practical duty of ensuring this contingency by every means in his power. And, taking our medicaments at their lowest value, there can be no doubt that their proper use tends to bring about this desirable result more certainly and quickly than would otherwise be the case; sparing suffering; abridging convalescence; and saving the many lives which would succumb before the arrival of less rapid and complete relief. Even in dyspepsia, the palliative effects of drugs are far too precious to be dispensed with. While as regards their less measurable tonic and alterative effects, those who have witnessed their influence among the dyspeptic poor, and against all the terrible concomitants—scanty food, foul air, excessive toil, and intemperance—dyspepsia here possesses, must know that it would be a mockery to restrict this important class of our patients to regimen or *hygiène*. To tell an idle and wealthy dyspeptic that his malady might ultimately be cured without drugs, would be a

hardy statement, but sometimes a true one. But to recommend dyspeptics in general to dispense with all medicines, would be not only illogical, but absurd: indeed, considering the circumstances of most of our patients even in the affluent classes, scarcely less foolish or cruel than to withhold a rope from a drowning man, and to advise him to save himself by building a boat."

Speaking of alcoholic and other stimulants—

"Secondly, as regards the general or constitutional effects of these substances, pray understand clearly that I neither deny nor affirm that there are grounds (moral, social, or what not) for separating and distinguishing their action; and that, in classing them together, I am doing so purely on such physiological grounds as would oblige me, if I were addressing a Chinese audience, to add opium to the list. Tobacco may be the exponent of all the vices; and tea a fertilising liquid which, applied in proper quantity, encourages the growth of all the virtues. While in respect to the ethics of alcoholic liquids, professors of Teetotal exegesis may explain away those opinions, as to the lawfulness and expediency of such drinks, which have been handed down to us from the wisest and best of mankind—from Paul, and Solomon, and a greater than Solomon—may shew that the text of the Evangelist is garbled, and (reversing both the miracle and its motive) turn wine into water to suit their narrow and gloomy views. But we have to consider such questions from a different aspect: and may sum up the chief effect of all these agents in the theory, that, despite great differences of detail, they have much in common. With little convertible material, little substance which the organism can assimilate, they all seem to modify nutrition in much the same way: diminishing the measurable waste of bodily substance; and especially reducing the excretions of carbonic acid and urea which chiefly express that waste, to a quantity below the respective amounts which would otherwise be habitually evolved."

"Without digressing any further toward this wide subject of the action of alcohol and its congeners, I may state that experience shews their local effect upon the alimentary canal and upon digestion to be, on the whole, unfavorable: while their general effect of altering (if not disturbing) the balance of nutrition, is often scarcely less detrimental to this function. That they are used by the majority of mankind, and that their moderate enjoyment is quite compatible with health, are facts which by no means countervail this proposition. And that the advantages of their general effects often preponderate over the trifling disturbance they produce locally—that, in short, they are sometimes useful remedies against the very ailments which their abuse (or even their moderate use) can otherwise bring about—is a statement which, while it involves no inherent improbability, rests upon an empirical basis such as defies disproof."

"As to the use of *alcohol* in dyspepsia, while freely conceding that every case is a rule to itself, and must never be decided on general principles, I would offer the following hints. First of all let me beg of you on no account to endanger the welfare of a patient who has been rescued by Teetotalism from death, by prescribing alcoholic drinks in any form whatever. I have known one or two awful relapses, and inconceivable misery (both to the patient and his family), produced by a

carelessness of this kind. It is much better that you should candidly inform such an one, that his laudable self-denial requires certain precautions; that it requires a parallel temperance in eating; and that, just as a moralist might doubt whether the reformed drunkard is entitled to substitute intemperance of thought and speech for intemperance of act, or to replace the privacy and penitence which befit a repentant debauchee by the public vituperation of what others have made a lawful and innocent indulgence—so the physician looks upon him as merely substituting one error for another, if, from drinking to excess, he takes to eating too large or luxurious a food. Indeed supposing the Teetotaler's avocations do not demand excessive and continuous toil, he digests so much more completely (at least such is my opinion) what he does take, that he requires less (rather than more) food than a person in affluent circumstances who moderately indulges in alcohol. While he certainly has lost one safeguard which alcohol affords against the immediate results of over-eating."

Of regimen—

"There are many other measures, such as we include under the term 'regimen,' which are of extreme value in the more protracted and obstinate cases of this distressing malady. The importance of fresh air, of exercise, of the proper regulation of the functions of the skin, of the observance of a period of rest after meals, of the repose of an habitually over-taxed brain, and various circumstances of like nature—claim attention under this head. So, also, much might be said of such remedies as foreign travel, mineral waters, and the cold-water treatment. Mineral waters as therapeutical agents can hardly be valued too highly. It is certain that many of them are sustained, without inconvenience, by stomachs far too irritable to support the artificial mixtures prepared by the druggists in accordance with our prescriptions; and equally certain that they introduce into the system considerable quantities of the very remedies (chalybeates, salines, and aperients) which we are compelled to resort to as remedies in many varieties of dyspepsia. And whatever their 'alterative' powers, the circumstances which attend their administration are scarcely less beneficial: and especially, permit the enforcement of rules of life, which we sometimes despair of our patients' following, so long as they pursue their ordinary avocations, and mix in their customary society. The effects of pure water (as applied externally and internally by the hydropaths) in dyspepsia have yet to be ascertained. Judging from the evidence at present accessible, it might sometimes be of benefit. But I need hardly say that, applied by persons who make it a system, and regard it as a cure for all diseases (at any rate as a substitute for most other means of treatment), we cannot expect that selection of suitable cases, which would render its results either safe to the patient, or conclusive to the profession. And from analogy and experience we may safely assert—that most (if not all) of the benefits ascribed to it are equally explained by the fresh air, mental quiet, early rising, simple food, and copious exercise, which the subjects of the hydropathic course adopt, in conjunction with packings and douches:—perhaps after having steadily disobeyed every such recommendation on the part of their more legitimate advisers for months and years before."

After this analysis it is scarcely necessary to add that in our opinion this essay of Dr. Brinton's is a really good book—a book which is at once philosophical and soundly practical.

On the Pathology of the Urine. By J. L. W. THUDICHUM, M.D., Lecturer on Chemistry in the Grosvenor Place School of Medicine. (London, Churchill, 8vo, pp. 429, 1858.)

It is not surprising that, at a period when organic chemistry is pursued with ardour and success, and with an enthusiasm which belongs to what may almost even now be considered a new science, so much attention should continue to be paid to the chemistry of the urine. It is a fluid complex in its composition, and ever-varying in its appearance under the several disturbing causes of disease; and from the earliest times the marked alterations which it exhibited in its physical properties under different circumstances, have offered to the physician the hope of estimating through its modifications the special characters of disease. Year by year, since the first accurate analysis was made by the celebrated Berzelius in the beginning of the present century, the chemistry of the urine has assumed increasing importance, and that in relation both to physiology and pathology.

It might be supposed that after the labours of such chemists and physiologists as Fourcroy, Vauquelin, Becquerel, Simon, Lehmann, Scherer, Vogel, Marcet, Prout, Liebig, Golding Bird, Bence Jones, and many others, that the chemical investigations of the urine were complete, and that except for the purposes of some special diagnosis, any further works on the chemistry of this excretion would be superfluous. At the same time, we do not hesitate to say that Dr. Thudichum's work, although it offers no new discovery, nor any novel methods of analysis, will nevertheless be welcome to the library of the scientific practitioner, and accepted as a very valuable contribution to the chemical literature of the day. The arrangement of the work is both lucid and methodical, and one of its great merits will be found in the clear and succinct description of each constituent of the urine—a description embracing its history and literature, its chemical composition, its physical and chemical properties, the most approved method for isolating or obtaining it, its decompositions, its form of crystallization, its physiological relations, the quantity discharged in health and disease, the nature of the deposits, its forms, if any; and, lastly, the pathological indications to be gathered from its presence.

The work comprises forty-six chapters. After giving the general physical characters of the urine, its clearness, turbidity, &c., with the colours and tints of the urine, which are illustrated by a plate of chromolithographs after Vogel, together with his table for the determination of the quantity of uræmatine in the urine, the author gives in detail the best methods for collecting the urine, with tables showing the total quantity of urine discharged in a given time in healthy individuals. The determination of the amount of solids in a given quantity of urine by direct experiment, and inferentially by calculations based on the specific gravity, are explained at length in the second chapter. Each of the

succeeding forty-four chapters is devoted to the consideration of a separate constituent, for to this number, and even more, may the possible ingredients of the urine extend. These may severally be considered as—

1. Those which are natural to the urine.
2. Those which are foreign to it, or superadded, as the result of disease or lesion of the urinary organs or passages.
3. Those which are accidentally derived from articles of food, drink, medicine, or poison.

The chapters on urea and uric acid, the chief and most characteristic proximate principles of the urine, are the most interesting and useful in the book. After giving a complete chemical history of urea, the various methods for obtaining it artificially, together with the various combinations it forms with bases, acids, and salts, the author proceeds to the diagnosis of urea in urine, and other animal fluids. He gives Liebig's methods for determining the presence of urea in fluids, and gives the preference to the nitrate of the protoxide of mercury process for ascertaining the absolute quantity of urea present, as recommended by that distinguished chemist. Dr. Thudichum speaks of this process as requiring no particular ability on the part of the operator, and recommends it for its ready applicability. In the hands of a practised chemist, and with all the appliances of a laboratory at hand, it is simple enough, but it is not so applicable in general practice, whether in hospital or private, as Dr. Davey's method, which the author gives at page 69. This method is founded on the fact that urea is very readily decomposed by the hypochlorites of soda, potash, or lime, and its constituent nitrogen evolved in the gaseous state. From the quantity of gas evolved, the amount of urea is estimated.

On the subject of the physiological or standard quantity of urea, the author says:

“Numerous experiments have shown that a healthy man, who lives well, discharges from 30 to 40 grammes of urea in twenty-four hours, which, calculated upon one hour, gives 1.25 to 1.66 gramme. This average must of course vary a little according to the size of the individual; and in the individual it must depend on accidental circumstances, which will sometimes change it, and in rare instances will produce extreme maxima and minima. But for practical purposes the above figures are valuable, even though subject to the variations mentioned. It would be a much better basis for comparative researches at the bedside, if the average amount of urea could be expressed in proportion to certain units of weight of the body, say pounds or kilogrammes, or units of measure of length of body, say centimetres. But many experiments will be requisite before such a basis can with safety be given.

“It would be useless here to enter into any speculation or theory on the mode and place of formation of urea in the system. We leave it undecided whether it is made in the blood, or blood-corpuscles, or in the muscles, however important the decision of these questions may be. But one great fact is undoubtedly established; namely, that as urea is the principal product of the metamorphosis in the body of nitrogenized food, the quantity of urea must stand in a direct relation to the quantity of food taken; or, if little or no food be taken, to the amount of nitrogenized component parts of the body disintegrated in the place of food.

In this sense must be taken the expression that urea is the measure of dissimilation, if I may be allowed to use this term as the antithesis of assimilation. 'Der Harnstoff ist das Maass des Stoffwechsels,' says Bischoff. And this is so nearly true that (with a slight modification of the sentence) we may say—the amount of urea is the measure of the most important part of the change of matter in the system. The *intensity* of the change is expressed by the *amount* of urea in the urine. More urea is produced during waking, than during sleep; more during bodily and mental exertion than during an equal period of inactivity. A large amount of nitrogenized food taken into the stomach will increase the amount of urea above the average; a small amount of vegetable food will make it sink below the ordinary medium.

"In calculating the discharge of urea with regard to time, we must take care not to lose sight of the fact that the production of a given quantity within a certain time may appear larger or smaller according to certain circumstances, which retard or facilitate the secretion of the kidneys. If the amount of fluid discharged as urine become very small, the amount of urea discharged in a given time will also be smaller too, owing no doubt to a part of the urea formed being retained in the system, particularly in the blood and muscles. If, on the contrary, the kidneys have to discharge a larger amount of water, the total quantity of urea will be raised above the average; because an amount of urea which, under ordinary circumstances, would be retained in the system for a time, is discharged with this increased bulk of water. This is the case for solids generally, and we may well say that a large quantity of water acts as a diuretic so long as there are soluble substances in the blood to be carried away with it."

Of excess or deficiency of urea in the urine, the author says:

"If the amount of urea remain above or below the average for any length of time, so that the possibility of an accidental variation is excluded, it is a symptom of disease.

"I will first consider the excess of urea. It is common in the *stadium incrementi* up to and over the acme of all acute febrile diseases, such as typhus* and pneumonia, &c.; and the total quantity of urea discharged in twenty-four hours may amount to 50, 60, or 80 grammes (A. Vogel), being double the amount of that discharged during health. This increase becomes a more important feature of disease, when the ingestion of nitrogenized matter falls to a minimum at the same time; in other words, because these patients have mostly no appetite, and if they have, are obliged to restrain it by the dietetic rules of their medical attendant. As soon, however, as the fever has abated, the amount of urea will sink: and that the lower below the normal quantity, the less food the patients are able to take from the continuance of loss of appetite, or from the inadequacy of the organs of digestion to perform their task. But as the patients recover appetite and strength, the amount of urea rises to its usual height. The same process is observed

* Dr. Alfred Vogel, *Harnuntersuchungen nach Liebig's neuer Methode*. Henle und Pfeuffer, 'Zeitschrift für rationelle Medicin.,' Neue Folge, Band i, 1854, p. 362. Also the same author's 'Monograph on Typhus.'

For some particulars, see the review of Dr. Stromeyer's work on Typhus, in 'Assoc. Med. Journal,' 1856, p. 46.

during the exacerbations of chronic disease, which in fact constitute an acute episode in the long train of symptoms. So an exacerbation of phthisis may be accompanied by urine similar to that of an attack of pneumonia, containing an excess of urea.

"But in diseases which are chronic and accompanied by impaired nutrition, the amount of urea sinks below the average.

"The lowest amount of urea which I have ever observed to be discharged by a patient during twenty-four hours, was 75 grains in 200 fluid drachms of pale, faintly alkaline urine. This was from a lady suffering from an ovarian tumour, for which she had been salivated several years ago. The growth of the tumour had been arrested since that time, but an anæmiated condition of the body had established itself, against which all treatment was tried in vain.

"So low an amount of urea as 75 or 90 grains in twenty-four hours generally only occurs towards the fatal end of diseases, when not only the production of urea is very limited, but also the excretory activity of the kidneys begins to become languid.

"The diminution of the quantity of urea may, however, be due to the failure of the excretory activity of the kidneys only, though at the same time an excess may be produced in the system. The excess is then retained in the blood, tissues, and juices of the body, and causes the cachexia commonly known as uræmia, which, if it be true that the products of decomposition of urea, namely, ammonia and carbonic acid, are the *materies et causa morbi*, had better be called ammonæmia. When urea is retained water is also mostly retained in part, and by its effusion into the cavities and cellular tissue, causes dropsical disease. Urea may be detected in most secretions, excretions, exudations, and effusions. It is the same with dropsical effusions from other causes: they contain in solution an amount of urea derived from the blood, but in these cases the impairment of the excretory activity of the kidneys is a secondary symptom, and scarcely ever causes that amount of retention of urea which may lead to uræmia. And even then the kidneys may be stimulated by diuretics or by exercise, or a spontaneous rally of the system may revive their excretory activity, when, with a large amount of urine, a proportionally large quantity of urea, which has been accumulated in the system, may be discharged. The amount of urea will here indicate the amount of depuration effected, just as in retention of urea the smaller amount discharged will allow us to calculate, taking the whole case into consideration, the amount produced, and, by subtraction, the amount retained in the blood."

The chapter on uric acid is worthy attentive perusal. After giving a general outline of the history and literature of uric acid, the best methods for obtaining it pure, and the sources from which it is usually derived, the subject of its crystallography is fully entered into.

The account of the salts of uric acid, the urates, those of ammonia, soda, and lime, follow.

Dr. Thudichum adopts Scherer's opinion, that the deposit of uric acid after emission, is the product of a peculiar kind of fermentation. He says:

"As uric acid cannot be secreted by the kidney from the blood in any other form than that of a soluble acid urate, it must always arrive as such in the pelvis of the kidneys. The intervention of a stronger acid

is now required to separate uric acid from its base. That the ordinary acidity of the urine should not be sufficient to effect this, nature has taken care to arrange. Whence, therefore, does the acid come which in urine of average description after some hours or days of standing produces a precipitate of uric acid? Scherer* has shown that this acid is the product of a peculiar kind of decomposition or fermentation of the urine, which he called the *acid fermentation* just because its main feature is the production of one or several acids. The ferment he considers to be the mucus, which causes the colouring matter of the urine to ferment, and to give off among other substances lactic and acetic acid. Under the influence of these acids, uric acid is precipitated sooner or later, according to the time at which the decomposition of urea begins to neutralize the acid formed. The decomposition of urea constitutes the *alkaline fermentation*, a process which we have already described in the chapter on alkaline urine. An important fact is, that if the alkaline fermentation succeeds the acid one, any uric acid precipitated is transformed into urate of ammonia. Of the alkaline fermentation I shall have to treat more at length under the section on triple phosphates."

The subject of uric acid concretions, and those of urate of ammonia, soda, and lime, comprehends the most recent views on the formation of these calculi.

The occurrence of the alternating layers of uric acid and urates in mixed calculi is thus explained :

" These layers are due to the same causes as the massive concretions. But there seems to be one cause of the occurrence of urates to which it is necessary to advert, before concluding the chapter on uric acid. Ammonia at the temperature of the body very quickly changes uric acid into urate of ammonia. Let us apply this to a uric acid stone. The urine in calculous disorders very frequently becomes alkaline; nay, if the disorder last long enough, alkaline decomposition of the urine in the bladder is almost always present. In evidence of this, almost one half of all calculi possesses a cortical layer of mixed phosphates, the consequences exclusively of alkaline urine. Now there can be no doubt, that if an uric acid stone become the cause of such disorders of the urinary passages as will induce alkaline fermentation in the urine, the outer layers of this stone must be transformed into urate of ammonia, and this urate of ammonia (or soda) would undoubtedly be dissolved and carried away, unless the carbonic acid present diminished its solubility, and if the urine were dilute enough to act as a solvent, *i. e.*, were not itself too much saturated with the urates. A stone with a uric acid nucleus, a body of urates, and a cortical portion of mixed phosphates, is to my understanding the type of this process, and suggests to me the following history : In the beginning there was a renal uric acid concretion, which increased by the acid fermentation in the bladder. Then the calculus caused disorder of the bladder, or of the urine (as by excessive treatment with alkaline remedies), which ended in the establishment of alkaline fermentation in the bladder, by which the outer layers of the uric acid concretion were first transformed into urate of ammonia, and afterwards encased in a crust of mixed phosphates."

* ' Annal. d. Chemie und Pharmacie,' Band xlii, p. 171.

In the chapter on creatine in the urine there are some interesting remarks on the pathological indications of this substance in relation to spasmodic and convulsive diseases. Creatine occurs in the muscular tissues of all vertebrate animals. It is most abundant in those in whom muscular exercise is greatest; it is, therefore, more plentiful in wild animals than in tame. In hunted animals, the fox, the deer, the hare, it is greater than in domesticated animals. The heart, a never-resting muscle, appears to contain a larger proportion than other muscles. Creatine is a constituent of the urine, and appears, therefore, as a strictly excrementitious product; it is a product of the chemical changes in the muscles. The pathological import of these facts, the author very properly suggests, may be made available in estimating the intensity of certain spasmodic diseases. Of the quantity discharged in twenty-four hours, he says:

"This question seems of sufficient importance; but few observations have been made on it. My own experiments, detailed at the end of this chapter, yield 0.305 gramme of creatine discharged in the urine during twenty-four hours, as the average of twenty-six days of two individuals. In disease the quantity of creatine, together with that of creatinine, might serve to indicate the intensity of any spasmodic or convulsive action. The question as to its quantity in tetanic and epileptic diseases is one of high interest. Cases of paralysis agitans, in which the spasmodic action ceases with sleep, may perhaps afford good opportunities for demonstrating the influence of rest and motion; though the different nutrition in the muscle may, perhaps, vary the chemical changes in some degree.

"These suggestions for future researches must not be mistaken for theories or suppositions."

The author's views on the subject of the colouring matter of the urine, the *uræmatine*, are identical with those of Vogel. Hippuric acid, chlorine and the chlorides, sulphuric acid and the sulphates, with phosphoric acid and its salts, occupy as many chapters, which are followed by one on the alkalies and alkaline earths, potash, soda, ammonia, lime, and magnesia.

The substances hitherto described are common to healthy urine, and it is their deficiency or excess which constitute a pathological condition.

The succeeding chapters, commencing with blood and its anatomical elements (chapter xvii), are devoted to substances which are foreign to healthy urine, and the mere presence of which imply disordered function or even serious disease.

In a work so eminently chemical, and so meritorious in this branch of investigation, it is no discredit to the author that those portions of the work which are more purely pathological, and which derive less aid from chemical research than from clinical experience, should leave on the mind a sense of deficiency, which is perhaps increased by the fulness and completeness of the chemical analyses.

In all that relates to the chemistry of the blood in the urine, the same completeness is manifested which graces other parts of the volume; and the same may be said of the chapter on albumen; but the pathological indications of blood in the urine, and of the presence of tube casts,

must be considered but as a summary of Dr. George Johnson's observations on these subjects.

The chapter on grape sugar contains very clear directions for the quantitative determination of grape sugar in the urine, and there is a very good description of the method of using Biot's polarizing apparatus as manufactured by M. Soleil, of Paris.

From a chemical point of view, the nature of those pigmentary matters which are occasionally found in the urine, and to which the names uroxanthine, uroglauine, anhodine, have recently been given, formerly known as cyanourine and melanourine, is still surrounded by obscurity. Dr. Thudichum brings the researches of chemists down to the present period, but leaves the pathological indications of these exceptional matters untouched. The limits of this notice do not permit us to do more than to commend the chapter on oxalic acid and the oxalates to the attention of pathological chemists, and to reiterate the opinion already expressed, that this work of Dr. Thudichum's, as a contribution to the chemistry of the urine, is the most complete and the most useful of any hitherto published in England. We cannot forbear expressing our regret, however, that the author should throughout the work have employed French terms for his measures of length, capacity, and weight. To many this will be a serious drawback. The work is addressed to English readers; and although to the practised analyst the conversion of these terms into English equivalents is an easy matter, yet the general body of the medical world, for whom the work is intended, will find the utility of the work lessened by the employment of French instead of English measures.

The Vegetable Parasites of the Human Skin. By MR. JABEZ HOGG, Assistant Surgeon to the Charing Cross Hospital. ('British Medical Journal,' 26th March, 1859.)

In this interesting and important communication Mr. Hogg proposes, *first*, to sketch the history of parasitic vegetable growths in diseases of the human skin; *secondly*, to inquire into the truth of the theory founded on the observations of foreign physicians, which attributes certain forms of skin-disease to the ravages of a vegetable parasite, as their origin, cause, and pathognomonic character; and *thirdly*, to offer a few remarks on the natural history of vegetable parasites, with a view to illustrate the subject by analogy.

It is now more than twenty years since Bassi of Milan discovered the vegetable character of a disease which caused great devastation among silkworms; and, about the same time, Schönlein of Berlin was led to the detection of certain cryptogamic vegetable formations belonging to the order *Fungi*, accompanying the development of certain diseases of the skin. The observations of this distinguished man have been confirmed by Gruby, Remak, Langenbeck, Robin, Küchenmeister, and other continental writers; as well as by Dr. Hughes Bennett, of Edinburgh, and Drs. Jenner, Gull, and other English physicians, most of whom have attempted to identify the fungus with the disease believed to be produced by it, and in this way to separate and detach some four or five of the

most common chronic skin-diseases from the rest, and to regard them simply and exclusively as *fungoid* or *parasitic diseases*.

This very tempting theory involves an important principle of pathology, inasmuch as it places the pathological fungi in a category by themselves, and invests them with characteristics entirely at variance with those of the natural history of the family of fungi, whose leading feature appears to be that of selecting disease and decay as the soil which is essential to their existence; whereas this hypothesis describes them as preying upon healthy organized matter, and thereby establishing specific diseases.

In examining into the truth or fallacy of this theory by the light of physiology, we must bear in mind that the surface of the human body is supplied with a delicate covering, one office of which is to excrete and another to allow the exudation of effete matter from the blood. The excretion consists chiefly of microscopic epithelial scales; and the exudation is composed of gaseous matter, which sometimes becomes condensed and dried on the surface of the epidermis. The scales are friable and separable under very slight friction during health; and the transpired fluid makes its free escape, under ordinary circumstances, without any assistance from without. But want of cleanliness, deficient exercise, and, much more frequently, a cachectic condition of the fluids of the body, interfere with the natural processes of elimination; and then the skin becomes diseased, and in this diseased condition is often infested with parasitic fungi, the filaments and spores of which are found imbedded in the epithelial scales of the epidermis, enveloping the hair-bulbs and often traversing the hair itself, which is then more or less abnormally secreted, and deficient in organic matter.

Is this diseased condition the result of the ravages of the parasite? or is the parasitic vegetation the result of disease? This is a question of some practical importance; for, if the disease is produced by the parasite, we have only to destroy the latter, and the disease will be perfectly eradicated, and cease. If, on the other hand, the parasite only acts the part of a scavenger, whose work it is to remove the *materies morbi*, then we have to rectify the condition on which the disease depends, and the parasite will starve for want of nourishment.

Authorities are not wanting on either side of the question. Dr. Hughes Bennett maintains that all such vegetable formations are secondary, and always arise in living animals previously diseased. Dr. Gull contends, on the contrary, that the parasite is the sole cause of the disease; and Dr. Jenner has committed himself to the same opinion. Mr. Wilson differs again, and entirely ignores the existence of vegetable parasitic growths in skin-diseases; and endeavours to prove—somewhat imperfectly, in Mr. Hogg's opinion—that the cylindrical tubes “seen under the microscope are nothing more than a splitting up of the middle or fibrous layer of the hair, and its oval-shaped cells, closely packed together, and arranged in a linear order.” Had Mr. Wilson submitted his specimens of disease to a closer examination, and employed polarized light with the microscope, he might easily have convinced himself that his view of the diseased hair was not quite correct.

The question, however, must be determined, not by authorities, but by further observations.

"During the last two years, with a special view to this question," says Mr. Hogg, "I have been engaged, with my friend Mr. Hunt, in making microscopical examinations of the products of cutaneous diseases appearing in the patients attending his practice at the Western Dispensary for Diseases of the Skin; and the result is, that I feel strongly inclined to the opinion that the vegetation found on the skin and hair is not primarily the cause, but rather the result, of disease.

"I will now endeavour to show on what grounds this conclusion has been arrived at.

"1. If there be any exceptions to the general law, that parasites select the subjects of debility and decay, such exceptions are not found among vegetations belonging to the order *fungi*, which invariably derive their nutriment only from matter in a state of lowered vitality, passing into decomposition, or wherein decomposition has already proceeded to some extent.

"2. That the growth of these fungi is not necessarily pathognomonic of a special disease, is obvious from the fact of their having been observed in nearly all kinds of chronic skin-disease. Thus, out of twenty cases of lepra and psoriasis, in which the products were examined, evident traces of fungoid vegetation were observed in ten. The same growth was noted in two out of three cases of lichen, also in four out of six cases of eczema, in one case of ichthyosis, and in one of spilus. None of these diseases have been supposed to be caused by fungi; and yet fungi were present in the greater number of the specimens examined.

"3. Other observers, some of them highly competent, have not been able to find them in the diseases they are believed to engender. Thus Malherbe, Cazenave, and Wilson, deny the existence of a vegetable fungus in *porrigo scutulata* (the common ringworm), although this is described and depicted under the name of *trichophyton tonsurans* by trustworthy observers. Cazenave, Didot, and Wilson, deny the existence of the *achorion Schönleini* in *favus* or cupped ringworm. Wilson and Cazenave deny the existence of the *microsporon Audouini* in *porrigo decalvans*. In reference to the statements of the latter observer, Cazenave, it must be borne in mind that he candidly acknowledges his ignorance of the microscope; and, not to make an unfair use of this negative argument, I must confess that I have seldom been disappointed in finding some kind of fungoid growth in all the diseases supposed to be produced by them. Nevertheless, such is the general similarity of form and growth of the fungi in the specimens examined, that I have failed to make out any relation between the special variety of parasite and the particular disease whose name it bears. Thus, in a case of *porrigo* in a girl of 16, which had existed for nine years, from neglect and dirt, I found, not a porriginous variety, but the fungus described by Robin and Küchenmeister as peculiar to *plica Polonica*—a disease almost unknown in this country. Again, in a case of *tinea tarsi*, I found sporules of the fungus described by Ardsten as the *puccinia favi*. Robin also found in *favus* the *puccinia* occurring together with the *achorion Schönleini*; the latter presenting itself as a constituent of the cups or crusts, while the *puccinia* occurs afterwards on the desquamation of the epidermis. And this is thought by some to warrant the opinion that the *achorion* is only the spermagonial form of the *puccinia favi*. But, further, it is broadly

asserted by others that the several morbid conditions observed in chronic diseases of the skin are mutually convertible; and that lichen, eczema, impetigo, psoriasis, lepra, mentagra, etc., are but modifications of one and the same disease, resulting from accidental conditions, and not always found perfectly distinct; nay, so often are they combined and complicated with each other, that dermatologists have assigned special names to some, indicative of their mixed character, such as *eczema impetiginodes*, *eczema leproides*, *lichen urticatus*, *erythema papulatum*, &c.

"It may be said, that most of the cases examined by Mr. Hunt and myself should be referred to the last-named forms of disease; and, therefore, the finding of parasitic fungi might have been, *à priori*, expected; indeed, ought to have been found. Having had this contingency in view, Mr. Hunt gave me specimens taken only from well-marked cases; and the morbid products of these were conjointly examined and sketched under the microscope.

"The objects were examined both wet and dry, with reflected and with transmitted light, rendered achromatic and carefully arranged to avoid error by distortion, &c., and with a magnifying power of from two hundred to four hundred diameters in almost every case. The products were obtained, in scaly diseases, by gently removing the half-detached scales; in most eruptions, by simply placing the discharge on a slip of glass; in diseases of the hair, either of scalp or beard, by uprooting the hairs, and examining with as little disturbance as possible.

"I will now enter on a short analysis of the cases examined.

"*Favus*.—At the head of my list stands *favus*—a disease better known to continental writers than to us, and said by them to be caused by the *achorion Schönleinii*. In this country it is so rarely seen, that neither at Mr. Hunt's Dispensary nor in private practice have I been able to meet with a case for examination; so that this disease, which we find most carefully described, and the vegetable nature of which Schönlein has the merit of having been the discoverer, as also of exhibiting the fungus in the form of mycelium-filaments and granulated stroma, I am compelled to dismiss with this brief notice.

"*Alopecia (porrigo decalvans)*.—In eleven cases of alopecia, the hair on the margin of the bald patch was submitted to examination under the microscope. In three of these cases, one of which was the product of syphilis, no fungoid growth could be found. In the other eight cases, filaments and sporules were most unmistakably present.

"*Porrigo scutulata (tinea tonsurans)*.—In nine cases of *porrigo scutulata*, the hair was examined. Fungoid vegetations, or vestiges of them, sometimes with sporules, sometimes without, were observable in each of these cases, but in three of them they were imperfectly developed.

"It may here be observed, that the filaments of the *trycophyton tonsurans*, said to be the cause of this disease, are described as found in the substance of the roots of the hair, and spreading longitudinally upwards; whereas the *microsporon Audouini*, the supposed source of the *porrigo decalvans*, forms a tube round each hair *outside* the follicle, not in the substance of the hair. I have not been able to verify these distinctions. On the contrary, on comparing many specimens of these diseases with each other, I have always found filaments springing up

from the bulb, and then growing up around or along the hair, sometimes longitudinally in bifurcating branches nearly straight, sometimes in tortuous or spiral forms, with or without spores. In both diseases, the bulbs of the hair itself were variously decayed and deformed.

"*Pityriasis versicolor* (*chloasma*).—Seventeen cases of this disease contributed specimens for examination, in *all* of which vegetations were observed, and in several of them the fungus named *microsporon furfur*, supposed to be the cause of the disease, was clearly identified. This fungus is said to have been discovered by Eichstädt in 1846, and is described as exhibiting spores piled up in groups or heaps; and, although Robin himself could not find this parasite, I acknowledge that the characteristic grouping of the spores had been distinctly marked in most of the specimens I have examined. But, although this piling up of the spores may be in some measure identified with the disease, it does not follow that the disease is caused by the fungus, which may merely find in this form of disease a suitable soil for its growth and fructification. The fungus may be destroyed again and again by soaking the skin with a nitrous acid or mercurial lotion; but, unless attention is paid to the state of the blood, no lotions will cure the disease.

"*Mentagra* (*sycosis menti*).—The hairs were examined in six cases of this disease, and in all were broken or bent, and covered by fungoid growths. The roots of the hair were closely invested with spores and filaments, spreading longitudinally outwards and upwards. Both the filaments and spores are described as larger than those of the *microsporon furfur*, and they are said to form a sort of vegetable sheath to the hair below the skin only. This latter description is graphic and true; but I should hesitate to admit that the mere *size* of a vegetation can entitle it to be considered a separate species; for it may depend on the age, the growth, or on some peculiarity of the soil, just as the *polypodium filix mas*, or common male-fern, may appear in a dry barren soil as a delicate plant, and yet, in a damp and shaded situation, with a congenial soil, it may assume the appearance of a gigantic shrub, several feet in height. Moreover, the hair itself, as well as its follicles and root, was found in several instances, exhibiting tufts of fungi growing on its surface.

"*Psoriasis*.—Thirteen specimens were examined. In seven of them, filaments and sporules were clearly discernible; in the other six, epithelial scales only. These filaments and sporules were in no respect distinguishable from those found in the various diseases already described, and to which they have been said to be peculiar.

"*Lepra*.—Seven specimens were examined: there were found filaments and sporules in five of them, and in two of them epithelial scales only. These vegetations also were very similar to those found in psoriasis—another name, in fact, for the same disease.

"*Ichthyosis*.—In one case of this disease, filaments were found, clearly showing that a congenital disease or malformation may, under certain circumstances, prepare a soil for fungoid growths.

"*Lichen*.—In two cases out of three, fungoid growths were visible, similar in character to those already described.

"*Spilus*.—In two cases examined, one exhibited very fine hairs, with peculiar masses of a fine pigment surrounding the shaft of the hair, and others were covered by granular matter; another had filaments of a

fungus, with a few spores scattered over the masses of epithelial scales.

"*Eczema*.—In four out of six cases, fungoid appearances were observable; in one of them also were masses of spores; and in another a cast of a hair was observable, the hair having escaped, and the cast being composed of beautiful filaments, which had embraced and encircled the hair precisely after the fashion described in porrigo decalvans, although there was here no baldness. In fact, the arms, and not the scalp, were the seat of the disease.

"*Tinea tarsi*.—In five cases of tinea tarsi, two out of which were associated with eczema in children, in three were found mycelia, filaments, and spores, associated with epithelial scales and granular matter; in one, an isolated spore of a fungus, described by Ardsten as that of *puccinia favi*, the spores of which are almost if not quite identical with those caught on my glass slides suspended in the air.

"As to the growth of these parasites on the *healthy skin*, most conclusive experiments have been made, the results of which go far to prove that the skin of persons in health and vigour does not afford the required conditions for them to take root in it; that inoculation succeeds only in those places where pustules have been previously formed. Remak and others repeated the experiment of inoculation over and over again, and found it always failed in the healthy; but that in certain exudations, in peculiar states of the constitution, or where disintegrated matters existed which had undergone particular chemical changes, the *achorion* might be made to germinate, and produce a further growth of that identical fungi.

"Seeing, then, that the fungi are characterised throughout nature by feeding on effete or decayed matter; that the fungi supposed to be peculiar to certain diseases of the skin, are also found in many other diseases of the cutaneous surface; that competent observers have not been able to find them in these peculiar diseases; that sporules and filaments described as the cause of one definite disease, have been found in the products of another definite disease supposed to have a peculiar and distinct parasite of its own; and that attempts have been made in vain to implant these parasites in a healthy skin; one cannot but conclude that special parasites, peculiar to, and productive of, special diseases, *do not exist*. And in this opinion we are, at least, confirmed by the therapeutical fact, that the alleged parasitical affections are rarely, if ever, cured by destroying the parasite; and that they *can* be cured by the due administration of appropriate alteratives and tonics which are capable of correcting the blood dyscrasia, which, in fact, originates the disease.

"These views are countenanced by distinguished pathologists. Professor Bennett writes of favus: 'I believe that the pathology of favus is best understood by considering it essentially to be a form of abnormal nutrition, with exudation of a matter analogous to, if not identical with, that of tubercle, which constitutes a soil for the germination of cryptogamic plants, the presence of which is pathognomonic of the disease. Hence is explained the frequency of its occurrence in scrofulous persons, and among cachectic or ill-fed children; the impossibility of inoculating the disease in healthy tissues, or the necessity for there being scaly, pustular, or vesicular eruptions on the integuments previous to con-

tagion.' And in some few experiments wherein it has been said that inoculation has succeeded in healthy persons, the following explanation may be offered: 'that the material in which the vegetations grow, may at the commencement, in a molecular exudation, be formed either primarily or secondarily. That is, there may be want of vital power from the first, as occurs in scrofulous cases; or there may have been production of cell forms, such as those of pus, or epidermis, which, when disintegrated and reduced to a like molecular and granular material, secondarily constitute the necessary ground from which the parasite derives its nourishment, and in which it grows.' (*Principles and Practice of Medicine*, page 807.)"

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"It cannot be denied that the single fact of the universality of distribution of the fungi, is of itself a very strong argument in my favour, and leads to the belief that they are ever ready to fix themselves where disease has set up disorganization, or where, from other circumstances, the powers of life have become enfeebled. Should it not be as I have stated, then I maintain it still remains to be ascertained how great a share these microscopic parasitic organisms have in the causation of disease.

"But if we now turn to the etiology of cutaneous diseases, we find this simple fact—that when the spores of the fungi have become fixed, they take their food from the tissues (soil) which surround them, and perhaps extrinsically from the surrounding medium (the air); and the fitness of the soil is doubtless increased by humidity, which greatly assists in their germination, and is particularly favorable to all parasitic growths. Do we not find here also, an example of the law which must be obeyed, in the lowest forms of vegetable life as in the highest, whenever plants are to prosper, viz., 'the choice of the locality depends upon the peculiar properties of the soil sought for or avoided by the various species of plants'? The species to which our attention has been particularly directed, make choice of the animal cutaneous surfaces; and a knowledge of all the circumstances which favour their growth, may enable us to arrive at some general theory of the treatment to be adopted. It appears that we have chiefly to look to a change in the soil in which they grow, and to bring that to a state in which they cannot thrive, to ensure the destruction of these parasites. What, may I ask, has been already done to effect so desirable an object?

"Mr. Hunt's experience—and this has been very large—proves that, as a rule, we must not rely upon local applications to effect the cure, or even arrest the disease, without at the same time endeavouring to produce a blood-change. To improve the general health is also of the very utmost importance, and this can only be done slowly and by the most judicious application of our remedies. Perhaps the best of all our therapeutic agents to employ in these diseases, one indeed based upon scientific data, both pathological and chemical, and for the safe and effective administration of which we are indebted to the researches of Mr. Hunt, is arsenic and its preparations. This subject appears to be a specific for most of the forms of skin-disease of which I have been speaking, although it is not always essential; for some cases of porrigo, mentagra, &c., even when infested with our parasites, have yielded to

cod-liver oil, preparations of iron, with purgatives or alteratives, as the case may be, without any topical application whatsoever."

On the Upodermic treatment of Disease. By Mr. CHARLES HUNTER, late House-Surgeon to St. George's Hospital. ('Med. Times and Gazette,' 5th March, 1859.)

Mr. Hunter's proposition is—That to produce an immediate or decided effect, no method is more effectual than the one originally recommended and long employed by Dr. Alexander Wood, of Edinburgh, in the treatment of neuralgia, viz., the subcutaneous or, as Mr. Hunter calls it, the *upodermic method*. He maintains "that upodermic injections act more quickly than the same medicine when taken by the stomach; that they also act more powerfully and effectively; and that frequently they produce the greatest benefit where one equivalent dose administered by the stomach is entirely useless, and even prejudicial." And, certainly, the following experiments and cases would seem to warrant in some degree, these conclusions:

"*Experiment 1.*—I injected a few drops of water containing one twelfth of a grain of the acetate of strychnia into the cellular tissue of a cat; in one minute it was tetanic, and in two minutes it was dead.

"*Experiment 2.*—I injected half a grain of morphia into the subcutaneous cellular tissue of a rabbit; it was completely comatose in five minutes, and remained so for hours.

"*Experiment 3.*—By the kind request of Dr. Page, I injected half a grain of the acetate of morphia into the arm of a girl aged sixteen, suffering from extreme chorea of all the muscles of the body; in two minutes all the muscles has ceased their irregular movements, and in two minutes more the girl was asleep.

"*Experiment 4.*—A man suffering from tic-doloureux was for some time subjected to the upodermic treatment; he almost constantly slept in from two to three minutes after the dose was injected at night.

"In several cats which I injected with morphia, the first symptoms showed themselves in a very few minutes.

"*Experiments 5 and 6.*—I injected three quarters of a grain of the acetate of morphia into the cellular tissue of a man with *mania à potu*. He got to sleep almost directly, and slept for many hours. I gave the same man the same dose (three quarters of a grain of morphia) in one ounce of water as a stomachic dose; it was one hour before sleep was produced, which, when obtained, lasted only about one hour and a half.

"*Experiments 7 and 8.*—I obtained a fine healthy rabbit, and, assisted by Mr. Venning, injected half a grain of morphia in about three drops of water, into the subcutaneous cellular tissue of the animal. It appeared slightly affected in two minutes, and fell comatose without a struggle in five. The pulse and respiration were very much lowered at the same time; narcotism lasted in this rabbit from six to seven hours; it appeared perfectly convalescent in nine hours.

"Some days after, Mr. Venning present, I again attempted to narcotize the rabbit by the same dose given by the stomach; a gum catheter

was used to make certain of the fluid entering the stomach: for a long time we awaited the result, but no narcotism followed; the rabbit never even went to sleep, nor did it bring up the morphia.

"*Experiment 9.*—I injected three quarters of a grain of morphia into the cellular tissue of a cat; it became preternaturally quiet directly after; in seven minutes it gave a convulsive start, its position previously having been fixed and uncomfortable; in thirteen minutes it uttered short sharp cries; muscles of the body rigid; eighteen minutes, muscles of extremities became rigid also; the rigidity of the muscles became more marked, until at forty minutes it was in a general tetanic condition, the body fixed, and the legs extended. The cat, contrary to expectation, recovered; but for many hours suffered from the morphia, which appeared to throw the animal into a state of drunkenness, after the muscular rigidity had passed off. Two or three other cats, with a similar dose, I have thrown into a state of strong convulsion, and even of tetanic spasm, by the injected dose of morphia.

"*Experiment 10.*—With the aid of Mr. Keal, I administered the same dose of morphia to the same cat as in experiment 9—by the stomach—the gum catheter being passed down the œsophagus. The cat was quite natural for five minutes, then became quiet, and was sick in eight minutes, with five or six spasmodic retchings; it began the purring which morphia causes in all cats in fifteen minutes; at twenty minutes and thirty-five minutes, it had one leg convulsed, and in one and a-half hours became very exciteable and constantly on the move, as if tipsy: this lasted several hours; but it never had the general spasms or rigidity which the injected dose produced.

"*Experiment 11.*—With Mr. Ash, I gave the same cat the same dose by the stomach in the same quantity of water, but this time warm (instead of cold, as in experiment 10). Sickness was again the first well-marked symptom of the action of the morphia; it took place in ten minutes; the succeeding symptoms were as in last experiment, but milder.

"As the question might arise whether the sickness in these last two experiments might not have been due to the mechanical irritation of the stomach by the catheter, we made some experiments to set it at rest. We went through the process of injecting at different times cold and warm into the stomach of the same cat with the catheter; no sickness nor irritation of any kind ensued.

"*Experiment 12.*—In a patient with hemicrania, no narcotic, except morphia, injected, will cause sleep. Opium and morphia have repeatedly been tried by the stomach; they never produce sleep, they always cause sickness."

II.

REPORT OF THE PROGRESS OF SURGERY.

Aneurism of the Right Femoral Artery cured by digital compression, with statistics of twenty-four other cases treated in the same manner. By Dr. Goss, Chief of the Surgical Clinic of the Jefferson Medical College, Philadelphia. ('N. Amer. Med.-Chir. Rev.,' Jan., 1859.)

PRESSURE made by the fingers would appear to possess some advantages over that made by apparatus. Thus, it is at once more speedy in its operation and less painful; it can be regulated more easily; in some instances it can be made to act upon the artery alone.

CASE.—A negress, æt. 32, with aneurism of the right femoral artery at the upper and inner portion of the thigh. After administering a grain of morphia, treatment was commenced on the 10th of June, 1858, at 10 a.m., by applying the thumb on the artery as it passes over the pubic bone. At the end of an hour the temperature of the limb was somewhat diminished. At one o'clock the patient complained of sickness at the stomach, which was relieved by half a grain of morphia, and in an hour and a half she fell asleep. During the afternoon she felt drowsy and did not complain of pain.

11 p.m.—She has been complaining of pain for the last hour at the point of pressure, whenever the assistants relieved each other; in other words, when the pressure was relaxed. The knee is the seat of tingling pain, which extends to the toes, and the whole limb is painful when touched.

Friday, 10 a.m.—The tumour feels very solid and cannot be emptied of its contents. There is no bruit; but slight pulsation and thrill. The point of pressure has begun to be painful, and, to prevent any irritation of the skin, moistened flour was spread over it.

5½ p.m.—The tumour is very hard, but not diminished in size. No thrill or bruit, and but very feeble pulsation. The compressed spot is quite painful. Treatment discontinued after 31¼ hours' employment. A grain of morphia was administered, and pounded ice in a bladder was applied, to be continued during the night.

Saturday 10 a.m.—The patient slept well last night. The tumour is softer, but cannot be emptied. The pulsations are stronger, and there is some thrill. The compression was renewed at twenty minutes to eleven o'clock, and in an hour she complained of great pain in the knee, which soon extended to the toes. At half-past twelve the patient took one grain of morphia; the pain

soon ceased, but occasionally returned. At half-past five half a grain of morphia was administered.

At 8 p.m. the point of pressure gave so much pain that a thin opium plaster was interposed between the skin and the finger, to avoid more irritation. This had a good effect in palliating the suffering.

10 p.m.—The patient complains of much pain throughout the whole extent of the tumour. The thrill has disappeared, and the pulsations are becoming feeble. The femoral artery is beating forcibly, and a collateral branch, about the size of the ulnar artery, can be felt beating at the inner and posterior portion of the tumour.

At 11 p.m. half a grain of morphia was administered; faintest perceptible pulsations.

At 12 $\frac{1}{4}$ all pulsations had ceased. The femoral still pulsated pretty freely. The compression was continued for another hour, at the end of which time the femoral was beating gently.

At 2 $\frac{1}{2}$ o'clock, being satisfied that nothing more could be done, and that the circulation in the tumour was arrested, the patient was left, half a grain of morphia having been administered.

For a few days after this event the patient had slight tingling pains in the limb. Up to the time of her departure her appetite was good, and she slept well and was comfortable in every particular.

In about a week a gum ammoniac and mercurial plaster was placed over the tumour, and her diet was restricted. The tumour gradually diminished in volume, and she left for home on the 6th of July, not quite a month after the commencement of the treatment. At the time of her departure the affected limb had diminished to exactly the same size as the sound one. The femoral artery was pervious to within an inch above the tumour, and she could walk with comfort.

On the 13th of October, she came to the city to report herself. The tumour had diminished to the size of a walnut and was very solid. The locomotion was perfect, and her health was good in every respect.

The following are condensed reports of all the cases which have been treated by digital compression, either alone or in conjunction with apparatus.

CASE 1.—In 1844, Mr. E. Greatrex was the first to employ digital compression, in a patient with popliteal aneurism of the right side. The compression was total and double; being made by an Italian tourniquet, alternating with the fingers of the patient. The compressor was placed over the artery below Poupart's ligament, and when the pain produced by it became unbearable, the patient relaxed the pressure for a few moments, at the same time applying his fingers very firmly to the artery just above the instrument, so as to allow of no blood passing through the tumour. The cure was effected in twenty-four hours. In this case, therefore, the fingers were but accessory to the instrument.

CASE 2.—In 1846, M. Vanzetti employed digital compression alone, in a case of popliteal aneurism, by means of assistants, at the hospital of Kharkoff, in Russia. This was maintained for two days, but was inefficient. The patient was cured by the ligature.

CASE 3.—In 1847, Mr. Tuffnell had, in a case of popliteal aneurism, been employing double and alternate pressure by apparatus. The upper pad giving rise to inflammation of the inguinal glands, the finger of the patient was substituted. This pressure was alternated with that of the instrument for twenty-

four hours, when the inguinal pad was again used. The cure was effected in seven days.

CASE 4.—In the same year, Professor J. Knight, of New Haven, had a case of popliteal aneurism, in which apparatus could not be borne at all. He cured the case in forty hours, by *digital compression alone*, made by intelligent assistants, upon the femoral artery over the pubes.

CASE 5.—In 1848, Dr. W. H. Van Buren employed, for thirty-six hours, digital compression over the external iliac, in a case of true inguinal aneurism. Being unbearable, the artery was ligated and the patient cured.

CASE 6.—One month later, in the same year, Dr. George Fox, in a case of large inguinal aneurism, applied digital pressure to the external iliac for ninety-six hours. On account of the fatigue of the assistants, and being unable to procure more, the pressure was discontinued, the condition of the tumour being much improved. Mechanical compression was then employed for eight days, and the patient was finally cured by ligation of the external iliac.

CASE 7.—Professor Willard Parker, in the same year, in a case of diffuse aneurism of the femoral artery consecutive to ligation, employed digital pressure for seventy-two hours with apparent success; but the pulsation having returned, compression was made by a weight in the groin for seven days, and a cure was effected.

CASES 8, 9.—In 1850, Dr. J. R. Wood treated two cases of popliteal aneurism by digital pressure on the pubes, alternating with Dupuytren's compressor about the middle of the thigh. In the first case the cure was effected in forty-eight hours, and in the second, in eleven hours and a half.

CASE 10.—In 1851, Mr. Norgate employed digital compression, in a case of popliteal aneurism, for twenty-four hours, having previously used bad apparatus for five days. This was inefficient, and the femoral was tied; the operation was followed by gangrene, necessitating amputation, but the patient recovered.

CASE 11.—In 1852, Mr. J. Monroe had a case of popliteal aneurism in which digital compression was kept up for three days by convalescent patients in the hospital. Mechanical compression was then substituted, and a cure resulted in sixteen days.

CASE 12.—In 1854, Mr. Wm. Jameson employed digital compression for a popliteal aneurism for four hours, when it was discontinued on account of the patient being irritable and rebellious. Mechanical compression was substituted, and a cure was produced in six days.

CASE 13.—In the same year, a patient, under the charge of Mr. Colles, cured himself, without assistance, of a diffuse popliteal aneurism, by intermittent digital compression, in seven days.

CASE 14.—During the same year, and under the same surgeon, a femoral aneurism was cured in about six days, by partial, irregular, alternating digital and mechanical compression.

CASE 15.—M. Vanzetti, in the same year, cured a popliteal aneurism, by digital compression, in forty-eight hours, various apparatus having been previously applied.

CASE 16.—In 1855, M. Nélaton, in a case of popliteal aneurism, tried mechanical pressure for sixteen days. This not succeeding, he substituted digital compression, which was maintained for ninety-four hours. The cure was not permanent. The limb was amputated, and the patient died of purulent infection.

CASE 17.—In the same year, the same surgeon employed digital compression conjointly and alternately with direct compression, with apparatus,

in a case of arterio-venous aneurism at the bend of the elbow. In several days the cure was effected.

CASE 18.—In the same year, M. Vanzetti had an intelligent patient with a popliteal aneurism. Understanding the plan of treatment, which was deferred for twenty days, he himself, during this time, made pressure on the femoral artery, which seemed to have a slight influence upon the tumour. The treatment was confided to six intelligent assistants, and in four hours the patient was cured.

CASE 19.—In 1856, M. Michaux employed intermittent mechanical pressure, in a femoral aneurism, for four days. Digital compression was then substituted, and in twenty-four hours the cure was complete.

CASE 20.—The same surgeon, in the same year, in a case of popliteal aneurism, first employed a tourniquet and then substituted digital compression. A cure was effected in seven days, the compression having been several times withheld.

CASE 21.—In 1857, M. Gherini employed digital compression, primarily and alone, in a case of varicose aneurism at the elbow. A cure was effected in three hours and a half.

CASE 22.—In the same year, M. Michaux, in a case of popliteal aneurism, employed digital compression, primarily and alone, first partial and then total, and effected a cure in twenty-four hours.

For further convenience of reference these cases have been drawn up in the form of a table, from which it will be seen that fifteen were successful and eight unsuccessful. The number of the different varieties and the result are shown in the following table :

<i>Variety.</i>	<i>No. of cases.</i>	<i>Cure.</i>	<i>Failure.</i>
Popliteal ...	15	10	5
Femoral ...	4	3	1
Inguinal ...	2	...	2
Arterio-venous ...	2	2	...
	23	15	8

Cures.—In five cases the digital compression was employed *primarily and alone*. Nos. 13, 18, 21, 22, 23.

In four cases digital compression succeeded after apparatus had been abandoned. Nos. 4, 15, 19, 20.

Five times digital compression was alternated with pressure by apparatus. Nos. 1, 3, 8, 9, 14.

Once digital compression succeeded when combined and alternating with apparatus and direct compression of the tumour. No. 17.

Failures.—In six cases digital compression was tried before any other means. Nos. 2, 5, 6, 7, 11, 12.

In two cases digital compression had been employed after apparatus had been abandoned. Nos. 10, 16.

“After examining these unsuccessful cases, the question naturally arises, were they all good tests of this mode of treatment, and would it not have been possible to have cured several of them had the treatment been continued for a longer time?”

“Of these eight cases, it is probable that in No. 7 the cure would have been complete, had the compression been kept up several hours longer ;

in Case 11, the pressure must necessarily have been imperfect, on account of its having been applied by convalescent patients in the wards of a hospital. In Case 12, it was abandoned after four hours, because the patient was rebellious. In Cases 5 and 6, the pressure was in the one unbearable, and in the other no more assistants could be procured; in both, moreover, the aneurism was inguinal, where, on account of its unfavorable situation, it is exceedingly difficult to maintain pressure, and consequently a favorable result could scarcely be expected. Cases 2, 10, and 16, are the only ones which we would regard as perfect failures. In examining the ultimate results of these unsuccessful cases, we find that in four the artery was subsequently ligated, and all but one were successful, Nos. 2, 5, 6, 10; two were amputated, Nos. 10, 16; and one patient died of purulent infection. In three cases, subsequent mechanical compression resulted in a cure, Nos. 7, 11, 12. In Cases 7 and 12 the tumour was so modified as to render a cure by apparatus effectual. In Case 11, digital compression rendered apparatus bearable.

“The length of time required for the cure of fourteen of these successful cases—No. 17 has to be rejected, as the period is so indefinite—averaged two days and two thirds. When the compression was primary and employed alone, the average was two days and thirty minutes; when double and alternate, three days and seven hours; and when employed after apparatus had been abandoned, the mean duration was two days and twenty-two hours. Let us compare these results with those of aneurism treated by mechanical compression alone. Thus, Mr. Hutchinson found in the London hospitals that, of twenty-six cases of femoral and popliteal aneurism cured by this method, the average duration was nineteen days, while in the Dublin cases the mean duration of treatment was twenty-five days.* M. Broca has found that in ninety-nine cases the length of time required for a cure was a little less than fifteen days.† At the present time we may therefore state that digital compression alone has effected cures in the shortest time. Next in order comes alternate digital and mechanical compression; and lastly, mechanical compression alone requires the longest time for a cure, although the duration of this plan is far less than that of the treatment by the ligature. The reason why mechanical compression has not succeeded in a shorter space of time, is on account of its mal-application in most cases; and when this point has been properly attended to, there can be but little doubt that a cure will result in half the time it will by the ligature.

“A striking point in these twelve cures is, that five were effected by pressure with the finger alternating with the use of the apparatus. In two of these cases the pressure was regulated and kept up by the patients Nos. 1, 3. In the remaining three, Nos. 8, 9, 14, the compression was made by assistants. As this plan is so simple, has been attended with the best results, affords such relief to the patient and operator, and has always effected a cure whenever employed, it should be preferred to all other methods.

“The case of Mr. Colles, No. 13, is interesting from the fact that the

* See Erichsen's 'Surgery,' second edition, pp. 522-23; London, 1857.

† 'Broca des Anéurysmes,' etc., p. 844; Paris, 1856.

patient cured himself in seven days, by irregular intermittent digital compression, the treatment being carried on without the knowledge of the surgeon.

"In M. Vanzetti's case, No. 18, the patient had kept up the pressure himself for twenty days previous to the interference of the surgeon, so modifying the tumour as to render the cure perfect in four hours.

"Thus it will be seen that in a little more than one fourth of these cases the patients had been, in a greater or less degree, instrumental in the cure, leaving the surgeon but little to do.

"Another interesting fact is, as M. Broca observes, 'that digital compression is an American procedure, and the principal merit thereof belongs, undoubtedly, to Professor Knight,' who was the first to cure a case by this method, unassisted by apparatus. Besides his case, six others have been operated on in this country, making seven of the twenty-three cases; four of which were successful."

A Guide to the Practical Study of Diseases of the Eye.

By JAMES DIXON. (12mo, Churchill, 1859.)

Mr. Dixon's little work is one of the best manuals that has yet appeared upon the extensive and important subject of which it treats. Without pretending to emulate the extent or depth of Dr. Mackenzie's 'Treatise,' or the systematic character of Wharton Jones's 'Principles of Ophthalmic Medicine and Surgery,' it yet presents in an agreeable and intelligible form those varieties of diseases of the eye which constantly come under the notice of every surgeon, whether engaged in hospital or private practice; the description being neither cramped with hard and technical phrases nor drawn out to an inordinate length, and the methods of treatment given being those for the most part which have been found serviceable by the author himself. Sometimes, indeed, we are disposed to think Mr. Dixon's style is too sketchy, as in the account of sclerotic inflammation; but, on the other hand, the several affections of the iris are remarkably fully and clearly described. In the 'Treatment of Syphilitic Iritis,' he says, "Invaluable, indispensable as atropine is in our examination of many morbid states of the eye, I do not regard it as of any service in iritis, for an inflamed iris loses its power of motion. Atropine must, therefore, be useless during the active stage of inflammation. At a later period, when the iris is beginning to recover its motory function, it may, I think, even do harm, and in the following way: when fibrin is poured out behind the iris (which no doubt happens in all cases of acute inflammation) the pigment-cells of the uvea become for a time firmly united to the capsule of the lens; and if when the iris is regaining its motory function a forced dilatation of the pupil be effected by the influence of atropine, some of the pigment may be detached from the posterior surface of the iris and left adhering to the capsule, forming those brown patches so familiar to us in patients who have suffered from iritis. Only get rid of the fibrin which is glueing the pigment cells to the capsule of the lens, and the iris is at once effectually liberated."

In giving an account of glaucoma, after an excellent description of both the objective and subjective symptoms of the disease, Mr. Dixon

enters into a discussion of the surgical method of treatment which has been so recently introduced and so extensively adopted by all who have devoted themselves to the study of the eye, and which, as it affords a good example of the general style of the work, we shall transcribe. "The treatment of glaucoma by 'Iridectomy' was published by Gräfe in the 'German Ophthalmic Journal,' of which he is co-editor, and was also brought before the Ophthalmological Congress which met at Brussels in 1857. An incision was to be made through the cornea, as close as possible to the sclerotic; a considerable portion of iris to be grasped by the forceps, drawn out, and cut off. When the nature of the operation came to be explained, one could not fail to be struck with the apparent absence of all causal connexion between the morbid changes of glaucoma and the means proposed for arresting them. How was general hyperæmia of an eyeball and the consequent changes of its tissues to be overcome by cutting out a piece of the iris? No satisfactory rationale of the operation was offered. We were told that 'intra-ocular pressure' was the cause of all the phenomena of glaucoma, without any very clear account being given as to what was pressed or what effected the pressure, and we were assured that the removal of a piece of iris, by taking off the pressure, would bring about restoration of sight. Now, it was obvious, that if the vessels of the eyeball were overfilled with blood, pressure must be exerted upon all the other tissues, and that the removal of a portion of the iris would, *pro tanto*, lessen the total amount of solid matter contained within the fibrous case of the organ. But unless the original cause of the hyperæmia were to cease, would not distension of the blood-vessels still go on, and soon reproduce the same amount of 'intra-ocular pressure' as formerly existed.

"Then with regard to the forms of glaucoma to which the proposed 'new operation' was applicable; clearly in old cases, where serous effusion had already taken place to such an extent as to separate the retina and choroid from each other, or when in addition to this effusion the lens had become opaque and adherent to the iris, no removal of 'intra-ocular pressure' could be of service. There remained then as subjects for the new operation, either those chronic cases in which the lens had retained its transparency and no general separation of the retina and choroid had taken place, or recent acute cases. The facility with which the operation of 'iridectomy,' as it has been called, can be performed, has led to its being practised in an immense number of cases, and were we contented with the array of so-called cures which have resulted, we should, indeed, believe that glaucoma, hitherto so hopeless a disease, had been brought as much under control as cataract itself. But a careful criticism will convince us that many of the 'cases of acute glaucoma cured by operation' were simply cases of acute inflammation of the sclerotic, implicating to a slight extent the iris and cornea, and attended with severe neuralgia and impairment of vision—cases which would have yielded to judicious treatment if no *iridectomy* had been performed.

* * * For myself, I may state that although I could not recognise as sound the theory upon which the operation was brought forward as a cure for glaucoma, I tried it in a series of carefully selected and well-marked cases of the following forms of disease: 'amaurosis with excavated optic nerve,' as Gräfe has termed a peculiar morbid con-

dition; chronic glaucoma, when the lens had not yet lost its transparency; and in cases of acute glaucoma, characterised by sudden impairment of sight, rapidly followed by inflammation of the eyeball, dilated and fixed pupil, severe neuralgia, and total loss of vision.

"In neither of the first two classes did I find, nor had I expected to find, any improvement to result, nor in the third class was sight restored, but the inflammation seemed to be arrested, and the neuralgia was either very much lessened or it wholly ceased. I cannot, however, attribute this result to the removal of a portion of iris, but mainly to the evacuation of the aqueous humour through the large corneal wound."

Perhaps the best written chapter in the book is that upon cataract, and the more so because the reader is not wearied with a detail of many methods, but the plan now usually adopted by all English practitioners is clearly and fully given, with such additional comments as may serve to elucidate particular points. In speaking of the operation by solution or absorption, he says, it may perhaps be considered as the most beautiful and perfect in the whole practice of surgery, and after describing the steps of the operation, he adds, "It is a besetting error of inexperienced operators to suppose that a cure will be hastened in proportion to the amount of lens they can break up at one time, but except with young subjects the very reverse is the case. The great rule to be observed is—not to oppress the eye with more broken-up tissue than the absorbing power of the organ is capable of removing rapidly, otherwise the little fragments act as foreign bodies; inflammation is set up, and all absorption is at once checked; the sclerotic and conjunctiva become injected; there is pain in and around the eye, with weeping, and considerable intolerance of light; the aqueous humour is turbid and the iris discoloured; the cornea appears hazy, and its surface dull and steamy.

"In such a case the eye may be lost by chronic inflammation unless it be forthwith relieved from the broken-up lens which is oppressing it. This may be done by making an incision near the margin of the cornea and introducing a small scoop or spatula, so as to allow the softened portions of lens to escape with the aqueous humour. An operation of this kind, if resorted to in time, may restore the whole eye to a healthy condition, the inflammatory symptoms rapidly subsiding, and the iris and cornea resuming their natural aspect."

In regard to the position of the patient in the operation for cataract by extraction, Mr. Dixon recommends the recumbent position, with the head slightly raised, in opposition to the continental method of operating with the patient seated, "for what anatomist about to commence a minute dissection would allow the preparation to be held in the hands of an assistant instead of placing it on a table?" In the performance of this operation it sometimes happens that a fold of the iris is cut through in consequence of the aqueous humour prematurely escaping. When this occurs Mr. Dixon observes that the band of iris between the two apertures must be divided so as to lay them into one, before proceeding to lacerate the capsule and attempt the extraction of the lens.

He dwells strongly on the necessity of performing the corneal section with slowness and care, avoiding as far as possible the too sudden escape of the contents of the globe, and he particularly indicates that the line of incision should be "at such a distance from the sclerotic as shall en-

sure both edges of the wound being of genuine corneal tissue; for wounds of the true cornea, provided their edges are in accurate opposition, have a peculiar readiness for uniting, which is not equally shared by that extreme marginal portion which blends with the sclerotic."

The capsule should be sufficiently lacerated to allow the lens easily to escape, then gentle and properly applied pressure should be made upon the globe. The after treatment is excellently described.

In conclusion, we can cordially recommend Mr. Dixon's work to those who wish to obtain a good general knowledge of the principles of ophthalmic medicine and surgery, and who do not possess the time or the inclination to study the ponderous volumes that have been written on this most extensive subject.

On Wounds and Injuries of the Eye.

By WILLIAM WHITE COOPER. (8vo, Churchill, pp. 330, 1859.

Amongst the many treatises which have been devoted to the consideration of the eye and its diseases, it is somewhat surprising that no English work has yet appeared which gives in full detail the various injuries to which it is liable. We have special treatises on iritis, on operations, on the ophthalmoscopic appearances of the eye, but not one upon wounds of this organ, and yet these are exceedingly common, are very dangerous to sight, and require more than ordinary acumen and circumspection in their treatment. Mr. Cooper's work is designed to fill this hiatus, and in the excellent arrangement of his subject, in the numerous instances which he has adduced of almost every variety of injury, most of which appear to have occurred in his own practice, and in the judicious methods of treatment which he suggests, he appears to have succeeded very perfectly. We recommend his well-written and beautifully illustrated work, not only to those who are especially engaged in the study of ophthalmic diseases, but to the profession at large.

The first chapter is devoted to a consideration of the results and treatment of foreign bodies in the eye. A singular list of the materials which may be found there is given, amongst which the following are enumerated: seed-husks, grit, insects, thorns, and splinters of wood, together with many substances which are thrown off in the practical operations of many trades, as in those of knife, fork, and razor grinders, blacksmiths, &c.

In all instances the examination of the eye should be early, careful, and complete, and the removal of the foreign body should be conducted as delicately and with as little injury to the adjoining parts as possible. The patient should be seated on a low chair, in a good but not too strong light, with the sound eye covered; then the lids being raised, the surface of the cornea and sclerotic should be closely examined; if nothing be visible, the eyelid should be everted, when the foreign body will generally be found upon its inner surface. A sharp or a round-pointed needle may then be used for the purpose of detaching it. Occasionally a pair of forceps may be used, and the piece of conjunctiva containing the foreign body may be snipped off.

A caution is given against mistaking the stain of a fragment of steel or iron for the particle itself, a mistake which is often productive of great

suffering to the patient, from ill-advised efforts upon the part of the surgeon to remove it. When the particles remain long impacted, serious effects may ensue. A very instructive case is related, which, as it gives a good idea of the results and appropriate treatment, we shall transcribe. "A pallid, sickly-looking shoemaker was struck by a particle of iron in the right eye whilst looking on at a blacksmith hammering at an anvil. Six weeks afterwards the condition of the eye was formidable. The palpebræ much swollen, the conjunctiva, sclerotica, and cornea, acutely inflamed. The foreign body lay imbedded near the centre of the cornea, surrounded by infiltrated pus, and the anterior chamber was half filled with pus. Neither iris nor pupil could be discerned. There were great pain and debility, with weak pulse and foul tongue. Mr. Cooper extracted the particle with a needle, and ordered two leeches to be applied, with a warm poultice and frequent fomentations. Three grains of calomel immediately, an aperient draught after six hours, and six grains of Dover's powder every night. Seven days after, the pus had entirely disappeared, and the iris and pupil had become distinctly visible. What congestion remained was of an asthenic character. He was ordered five grains of cinchona bark and five grains of bicarbonate of soda, three times a day, with nutritious diet; the bowels to be regulated with sulphate of soda, and a blister to be placed behind the right ear. Seven days after he was quite well, with merely a slight film on the cornea."

The next chapter is occupied with the subject of foreign bodies in the eyeball, and the method of treatment in such cases. The injuries produced by fragments of percussion caps, especially those of foreign manufacture and those which are used at fairs in shooting for nuts, and which are not split, are of this nature. The apparent wound in the cornea is usually clean and small, and the foreign body may be either located in the anterior chamber, imbedded in the iris, or lodged in the substance of the lens, whose peculiar and increasing density usually arrests its further progress. Smooth metallic substances may remain in any of these positions without being productive of material distress or inconvenience to the patient for considerable periods. Far more frequently they produce more or less inflammation of the internal structures of the eye, and if lodged in the lens commonly produce cataract or opacity of that body. In other cases, besides blindness, severe neuralgia and sympathetic irritation and inflammation of the sound eye often ensue.

In some instances, where the fragment has penetrated deeply, it is impossible to discern it by mere inspection, and several instances are given where large portions of metal had been removed from eyes in which no foreign body was known or supposed to be present. In such instances the ophthalmoscope has been of great service.

Mr. Cooper considers that in all instances the patient should be placed under the influence of chloroform, and the foreign body removed with the least possible delay. At the same time he acknowledges that it is often a matter of extreme difficulty, requiring the utmost skill and patience on the part of the surgeon, especially where it is impacted in the substance of the iris. Where the lens is penetrated, it should be removed entire with the foreign body, as it is almost certain to become opaque.

A long section is taken up with gun-shot wounds of the eye, and many illustrative cases are given in point. A shot striking the eye may either cause simple bruising and concussion, or it may glance off, cutting a groove without penetrating, it may pierce through the tunics and lodge in the globe; or lastly, it may traverse the eye and bury itself in the orbit. In all cases the first thing to be done is to carefully cleanse the eye, if there be either blood or dirt, and then a full and satisfactory examination should be made to ascertain the nature and extent of the injury. In determining the question as to whether or not a shot has pierced the eye, the chief points to be considered are: the weight of the shot, the distance from which it was discharged, and the position of the eye, with the direction from whence the shot came. A guarded prognosis should always be given, even where there has apparently only been a slight graze. If a shot has passed through the cornea, there will be a visible breach of surface in the cornea, and the iris will be in contact with it; this obliteration of the anterior chamber is positive proof of that cavity having been opened. If the shot has wounded the iris, there will probably be some blood in the anterior chamber. If the lens be lacerated, in a few hours, it will in all probability be opaque. When a shot lodges in the posterior portion of the eye, it gives rise to a series of changes, the result of long-continued inflammation. The iris changes in colour, the globe becomes atrophied and soft, and the sclerotic puckered, the puckers running from the cicatrix. As regards treatment, the strictest antiphlogistic measures should be enforced. In the first instance cold-water dressings should be applied, if suppuration take place, fomentations must be substituted; and if there is much discharge, the dressings should be frequently changed.

The chapters upon incised and punctured wounds of the eye, with their usual consequences—prolapse of the iris, opacity of the lens, atrophy of the globe, staphyloma &c., are excellently written. The treatment, in particular, is very fully given, and many valuable suggestions are advanced, great stress being most properly laid upon a due attention to the constitutional state of the patient.

We regret that we have not space to follow Mr. Cooper through the interesting subjects of dislocation of the lens—the removal of which he uniformly recommends—and of intra-ocular hæmorrhage, except to mention that effusion of blood behind the lens is much more dangerous to sight than effusion into the anterior chamber, and also the great value of the ophthalmoscope in the diagnosis of these injuries.

The section on burns and chemical injuries contains by far the best description we have yet seen of these most formidable and destructive accidents; and both here and in the rest of the work we have particularly noticed the absence of hard and technical terms, the whole being written in a very agreeable, easy, and intelligible style.

In the last chapter Mr. Cooper enters into a consideration of sympathetic affections of the eyes, a subject which, since the general adoption of Prichard's plan of extirpation of the affected eye as soon as there is clear evidence of the sound eye being sympathetically affected, has assumed considerable importance. "The ordinary characters of sympathetic inflammation are, dimness of sight, passing into blindness; at first the appearances are by no means indicative of the formidable nature

of the disease; there is sclerotic redness round the cornea, effusion of lymph upon the capsule of the lens, and gradual discoloration and irregularity of the iris, which becomes bound down to the capsule. These effusions taking place in the posterior chamber, bulges the iris forwards, the vitreous humour becomes fluid, the eye loses its consistence, the retina its sensibility, until at length atrophy and shrinking of the globe, with total blindness, is the sad result.

“Wounds from foreign bodies most commonly give rise to this affection, and it is especially observed in those who make use of their eyes at too early a period after accidents.

“Ordinary treatment is here useless. Mercurials, iodide of potassium, and bloodletting or blister, are of little service. Occasionally a seton in the temple has been found useful, but upon the whole, extirpation of the affected eye is in general absolutely requisite.”

Practical observations on the Radical Cure of Inguinal Hernia. By C. HOLTHOUSE, F.R.C.S., Surgeon to the Westminster Hospital. (12mo, Churchill, 1858.)

Mr. Holthouse, in giving a *resumé* of the various operations that have been devised for the radical cure of inguinal hernia, gives the following classification: 1st. Operations for removing or obliterating the hernial sac by means of the knife. 2d. Operations for obliterating the hernial sac by exciting inflammation and adhesion of its contiguous surfaces. 3d. Operations for closing the hernial passages by a plug of integument. 4th. Operations for blocking up and closing the external abdominal ring. He then gives in full detail the particulars of Langenbeck's, Gerdy's, Wutzer's, and Mr. Wood's operations, with the mode in which a cure is effected, and statistical results in each case. Wood's operation, of which Mr. Holthouse speaks in terms of commendation, “consists in a kind of subcutaneous separation of the superficial fascia from the cord, pushing the former into the inguinal canal, drawing together by ligatures the two pillars of the external abdominal ring, and applying pressure over the opening.” As showing the frequency with which cases of hernia come before the surgeon, and the perfect cure that may in many instances be effected, this short and clearly written pamphlet should be in the hands of every member of the profession.

Practical observations on the Operations for Strangulated Hernia. By J. H. JAMES, F.R.C.S., &c., Consulting-Surgeon to the Devon and Exeter Hospital. (Pamphlet, Churchill, 1859.)

It is much to be regretted that those surgeons who have long held important posts in our large hospitals do not, at the close of their career, give the results of their experience in the form of tabulated extracts from their case-books, upon those subjects to which they have specially directed their attention, or on which there is much difference of opinion. Such tables would possess a double value, both as proceeding from unquestionably authentic sources

and also as representing the results of particular lines of treatment, on an average series of cases, in the hands of men for the most part thoroughly acquainted with every department of surgery.

Mr. James's work is of this nature, and it will be read both with profit and pleasure. We trust that it is only an instalment, and that he will not think it too great a labour to place before the profession, in a similar form, the results of his matured experience upon other matters.

Mr. James has operated on 36 cases. Of these 22 were femoral, 14 inguinal and scrotal; 24 were in hospital, 12 in private practice. Of the latter 6 died, of the former 8. Alluding to the occasional difficulty of diagnosis in females, between inguinal and femoral hernia, he points out that the cause of the obscurity arises from the fact, that the tumour mounts nearly to the spine of the ilium; when this is the case it may safely be pronounced to be inguinal. He considers that tobacco may sometimes act advantageously when chloroform has failed, not only from its power of unloading the capillaries, but also from the remarkable influence on the peristaltic motion of the lower bowels which tobacco possesses.

With only one exception, Mr. James has always followed the old plan of opening the sac, and he thinks that this mode of practice, in both kinds of hernia, will continue to be practised, at least in the majority of instances.

In all the cases of femoral hernia the stricture corresponded with the edge of Gimbernat's ligament, and was relieved by its division. "Having inserted the very point of my finger, I pass a narrow, strong, probe-pointed bistoury, guarded very nearly to the end, taking care that its edge is sharp. Passing this with its flat side, as soon as I have got its extremity into the edge of the stricture, I turn it, press it back into the pulp of my finger, press the point of that, so armed, against the edge of the stricture, carefully avoiding any sawing motion, but cut, as it were, with the finger itself; this safely divides a small notch, and allows the finger to be passed sufficiently far to protect any subsequent division which may be required."

As regards after-treatment, he thinks it rarely desirable to give aperients by the mouth, but that the action of the bowels should be solicited by enemata. When the action is antiperistaltic, opium becomes the best purgative.

Peritonitis is the usual cause of death; it is for the most part of a low type, and is often maintained by the continuance of an internal strangulation, either complete or incomplete—it may be, from the effects of an unrelieved stricture in the sac.

There is great "frequency, severity, and fatal tendency to erysipelas and gangrene, especially of the scrotum, after the operation for hernia."

A very excellent *resumé* of his own operations is given, and Mr. James concludes with an estimate of the value of Petit's operation, to which, upon the whole, he appears to hold an adverse opinion, except in certain very favorable cases.

The pathology and treatment of Stricture of the Urethra. By JOHN HARRISON, Esq., F.R.C.S. (8vo, London, J. Churchill, pp. 110, 1858.)

During the last few years several very complete works on stricture have issued from the English medical press, and from these the advanced student may obtain an almost exhaustive account of the nature and treatment of this disease. The little volume of Mr. Harrison does not pretend to enter into a complete exposition of all the minute details which are connected with stricture, but it is the work of a good practical surgeon, in which the author states what he has found to be the most important facts in a disease to which he has devoted much attention. As such it will always possess value, even to those who have been long engaged in the pursuit of their profession; but since, in the study of every subject, it is necessary that the chief outlines and fundamental propositions should first be known, from which the minor details may be readily filled in, so we think that Mr. Harrison's little book will prove of most value to those who, not desiring to make a specialty of this subject, are yet anxious to know the opinions and practice of a sound surgeon upon the chief points connected with it. To such we can strongly recommend this work.

Lettsomian Lectures on Syphilis. By VICTOR DE MERIC, Esq., Surgeon to the Royal Free Hospital, and to the German Hospital, Dalston. (Pamphlet. London: John Churchill, October, 1858.)

The first of these lectures treats of the unicity or duality of the virus of syphilis; the second, of some peculiarities of certain constitutional symptoms of syphilis, and the non-contagious nature of the whole series; and the third, of hereditary syphilis.

In the first lecture, the author urges the importance of the study of syphilis, and endeavours to remove some of the prejudices against specialism, by showing that a thoroughly educated specialist is a very useful member of the profession. After mentioning the now existing differences of opinion on syphilis, Mr. De Meric, in alluding to the origin of the disease, suggests that Ricord might be right in thinking that glanders were no stranger to it. As to the word *syphilis*, it should, according to the author, be restricted to the general effects of the disease on the organism; and he adds that the existence of a virus in this complaint is as certain as in scarlet fever, influenza, rubeola, and typhus.

The author holds that the syphilitic virus can but once be implanted in the economy, and that the receptivity ceases after a first injection. The essence of the poison is unknown, and no clue has as yet been found by the microscopical examinations of the virulent pus, conducted by Mr. Jabez Hogg, at the author's request. Ricord admitted, until lately, but one virus, supposed to give rise to soft and hard chancres, the first being exclusively a local disease, the second giving rise to systemic syphilis. Influenced by the researches of his pupil, M. Bassereau, who

considers that each of these chancres owes its origin to a distinct virus, Ricord has encouraged the investigations of another of his pupils, M. Fournier, and has *almost* been convinced of the truth of M. Bassereau's doctrine. The mode of investigation consisted in tracing the origin of contamination in a certain number of cases, which investigation brought to light the fact that one kind of chancre invariably generates its like.

Sketches of the doctrines of Hunter, Abernethy, Carmichael, and Wallace, as bearing on the plurality of the virus, precede the account of the rise and progress of duality propounded by Bassereau, long mentioned as probably true by Ricord, but not as yet freely and unreservedly admitted by the latter. The author has "endeavoured to add a few confrontations from his own practice," and concludes that "it is plain, that if we admit the doctrine of duality, we may do so without infringing upon the unicity of the *syphilitic* poison; because, from all we have said, it would appear that there seems to be a duality of *chancreous* poison, but that there is but one actually *syphilitic* virus capable of infecting the economy."

In the second lecture the author endeavours to ascertain the average of the time which separates the primary symptom from the eruption, taking as a basis the cases which have come under his care, and where no treatment had been used. The result of the inquiry is as follows: "We may, regardless of the *kind* of eruption, reckon a mean of six weeks, where no treatment has been resorted to." A simplified classification of syphilitic eruptions is then attempted, grounded on the tendency or the absence of tendency to ulceration.

First division.

Non-ulcerative or deciduous . . . Erythema and papules.

Second division.

Ulcerative or secreting . . . Vesicles, pustules, and tubercles.

The author adds: "I must confess my partiality for the practical lucidity which is afforded by this division, because ulceration denotes a considerable activity in the poison, and a want of resistance in the patient; hence, when we see the possibility of the formation of ulcers by the appearance of vesicles, pustules, or tubercles, we have made a discovery which is of paramount importance in the treatment."

The diagnostic value of the copper colour, and the absence of pruritus in syphilitic eruptions, is then shown to be unimportant; and condylomata (which Mr. De M. calls muco-cutaneous papules) are proved to be secondary and not primary symptoms, as maintained by some surgeons.

The next subject of which our author treats is the distinction between secondary and tertiary ulcers—a distinction of great interest, as the treatment should vary in the two series of ulcers.

The last topic of this lecture is one which has given rise to very animated discussions, viz., the question of contagion respecting secondary symptoms. The author belongs to Ricord's school, and thinks them not contagious; but we shall see how Mr. De Meric will view the subject in his further publications, as M. Ricord declared, on the 31st May last, at a meeting of the Academy of Medicine of Paris, that recent experiments have led him to modify his opinion.

The third lecture is taken up by researches of the author on two points of hereditary syphilis. These are: 1st, The limitation of time as to the appearance of the symptoms; and 2d, the action of the infected fœtus upon the healthy mother. He finds, from his cases, that in only two instances, out of a great many, the infected child showed symptoms at birth. As to the limitation of time, it is as follows: "I was able to ascertain, in twenty-eight cases, at what period after birth the symptoms had appeared; and I find, irrespective of the kind of symptoms, that they were noticed a few hours after birth in two cases, a few days after birth in four cases, from ten days to three weeks after birth in five cases, and from six weeks to thirteen weeks after birth in ten cases. We have thus twenty-one cases in which the symptoms appeared before the child was thirteen weeks old. The remaining seven cases are examples of the appearance of syphilitic symptoms at periods after birth ranging from ten months to fifteen years. The seven cases run thus: in the first, the symptoms appeared thirteen weeks after birth; in the second, fourteen months; in the third, twenty-one months; in the fourth, two years and three months; in the fifth, eight years; in the sixth, twelve years; and in the seventh, fifteen years.

This lecture closes with the relation of cases which would tend to show that a healthy mother may escape being contaminated by an infected fœtus, the author saying—"I have carefully computed the forty-four cases to which I have before alluded, and I find that in twenty-one of them, both parents were diseased before the birth of the child. These cases, therefore, do not bear upon the present question. But, out of the twenty-three remaining cases, I perceive that in thirteen the mothers who had given birth to syphilitic children remained in perfect health." These cases which would tend to invalidate the doctrines of Mr. Hutchinson, M. Devay, and M. Ricord, are then given in detail.

Lectures on Chancre. By M. RICORD. Translated by C. F. MAUNDER, Esq., Demonstrator of Anatomy in Guy's Hospital. (8vo, Churchill, 1859.)

Few subjects have raised a greater number of disputed points within the last few years than syphilis. The French, by reason of their stringent police regulations, and the large hospitals devoted to this disease, present singular advantages for the study of its protean forms; and it is accordingly not remarkable that numerous essays and treatises have appeared, in which it has received the fullest attention. Amongst the writers on this subject M. Ricord stands pre-eminent; and Mr Maunder has done a great service in translating, and in translating so well, the work of this distinguished surgeon.

M. Ricord clearly holds the doctrine of the duality of the chancreous poison: one producing the simple, non-indurated, cleanly cut chancre; the other producing the indurated chancre—the latter capable of infecting the constitution, the former incapable. He recognises two forms of bubo that may accompany the simple chancre: the one consisting of a simple, inflamed gland, or sympathetic bubo, susceptible of resolution, or suppurating without virulent specificity, and analogous to

those which might follow any ordinary wound; the other being a specific bubo, a true glandular chancre, *suppurating surely*, secreting an *inoculable pus*, and converting the wound following the opening of the purulent focus into a chancre.

In the treatment of simple chancre, M. Ricord recommends its early and complete destruction by means of the application of a paste made of sulphuric acid and vegetable charcoal, and the administration internally of the potassio-tartrate of iron, which may also be applied as a lotion.

Mercury should never be employed.

On the other hand, the indurated or Hunterian chancre, with its well-known characters, is uniformly accompanied—usually before the expiration of a fortnight—by hard, painless, indolent swellings in the groin, consisting of enlarged glands, which show no tendency to inflammation or suppuration, unless under the influence of extraneous exciting causes—the pus in such instances being simple, ordinary matter, without any virulent properties. “The drama of syphilis,” that is, of the constitutional affection which succeeds indurated chancre, may be said to be “divided into three acts or periods: 1st period. Primitive affection, the chancre, the inevitable source of *acquired* syphilis; the chancre with its faithful companion the bubo. 2d period. Secondary affection, opening the scene to the constitutional symptoms of syphilis, *i. e.*, succeeding the chancre within the *first months* of its existence; affections of the superficial tissues”—general lassitude, neuralgic and rheumatic pains, cervical adenopathy, alopecia, cutaneous eruptions of exanthematous kind, &c. “3d period. Tertiary affections, only manifesting themselves at a distant epoch from the original affection, rarely sooner than six months; affections of the deep tissues.”

The primitive affection is the only one which is incontestably contagious, and since indurated chancre creates a diathesis, it is necessarily subject to the laws of similar affections; it never relapses, and syphilitic diathesis is no more reproduced than any other kind of diathesis. It is clearly hereditary, but the blood of the affected individual, which is evidently the vehicle of poison, since it radiates it through his system, is not contagious, and cannot communicate the disease to a healthy subject, nor can a child receive the poison of syphilis from a nurse through the medium of the milk. A child infected after birth is infected by a chancre precisely in the same manner as an adult.

When the pus of an indurated chancre acts on an organism previously infected, it produces, not a hard chancre, but a chancre with a soft base, similar in appearance and form to the simple chancre; and from recent investigation it seems probable that the virus of this apparently simple chancre may, when affecting a perfectly healthy person, reproduce the hard chancre, but is also sometimes transmitted as a soft chancre. In regard to the treatment of indurated chancre, the great fact to be remembered is, that its destruction by caustic, excision, or otherwise, before the fourth day, that is, before its base becomes indurated, presents the manifestation of syphilis. When induration is fairly present, cauterization is useless—the syphilitic diathesis is acquired. For the treatment of that diathesis mercury is the chief remedy; but in its administration, the diagnosis should first be surely made, and it should be remembered that the curative action of mercury is generally *suspended*

from the moment that the morbid symptoms which especially belong to this agent begin to be produced. In the treatment of the secondary affections, iodide of potassium is absolutely requisite; but M. Ricord gives it in minimum doses, three or four scruples a day to begin with, subsequently increasing it to five, six, or eight scruples, and Mr. Maunder mentions yet larger doses having been given experimentally.

On Voltaic Narcotism for the production of local anæsthesia for surgical operations. By Dr. BENJAMIN W. RICHARDSON, Physician to the Royal Infirmary for Diseases of the Chest. ('Medical Times and Gazette,' February 12, 1859.)

Some six months ago Dr. Richardson published in the 'Medical Times and Gazette,' a paper on electricity and local anæsthesia. In that paper, which has since been often referred to in the late controversies on so-called "electrical anæsthesia," he tried to prove by experiment that the electric current in no one of its applications could be made to produce insensibility to pain. He explained that the intermittent current, inasmuch as it produces counter shock, might, in sudden operations, such as tooth-extraction, remove the idea or the consciousness of pain caused by the operation, by a diversion of sensation. His experiments up to that time were conclusive in so far as the intermittent current was concerned, and in so far as related to the effects of powerful electrical discharges. They required to be strengthened, however, in respect to the influence of the continuous current—a requirement which he took ready pains to fill up; the result being, that the continuous current, however used, is incapable of producing any degree of anæsthesia in such way as to be applicable as an adjunct to surgical art.

Laying aside for a time experimental work with the electrical force, he began in September last to inquire as to the possibility of producing anæsthesia by other means. The phenomenon known commonly as the going to sleep of the limbs was first passed in review, and he found, after a little trouble, a method of placing his leg in such a position as to induce this phenomenon in a marked degree. But the result was *nil*; for he found, however thoroughly the limb was numbed by this process, that, although it might feel a dead weight and its muscles might disobey the will, that externally it was sensitive to the slightest impression, and that the needle point pushed into the skin created just the same amount of pain as in other and unaffected parts of the body. The phenomena produced by this process are in fact closely analogous to those produced in a part by the transmission through it of a rapidly intermittent current of electricity; there is first, the tingling sensation, and secondly, the sensation of weight and so-called "sleep;" but the cutaneous sensibility remains the same.

He next turned attention to the application of the more powerful narcotics. The well-known fact that the tincture of aconite when applied to the lips produces a sensation of numbness presented itself naturally as open to inquiry; and he found that aconite and some other narcotics did produce a local insensibility which might possibly be turned to account in minor or superficial operations. But in the end he

discovered that this method was very limited in its effects; for, even in so thin a structure as a rabbit's ear, he could not, by the external application of narcotic solutions, produce any such degree of insensibility as to warrant him in proposing this plan as an anæsthetic process.

While these experiments with narcotics were under consideration, a thought occurred to him whether the combination of electricity with a narcotic would be of any avail. On the 31st of October he placed a sponge dipped in a solution of morphia on the arm of Mr. Gregson, and covering this with a copper plate connected with the positive pole of a small voltaic battery, and bringing the negative pole with a moistened sponge a little lower on the limb, he caused the current to pass, and produced in the course of a quarter of an hour a condition which he never obtained before; for on removing the poles he found that the part over which the narcotic had been applied was pale in colour, and, unquestionably, insensible to pain. Pricked with a needle Mr. Gregson experienced no sensation; and although the experiment was very imperfect, and its effects transitory, it was sufficiently important to encourage further researches in the same direction.

Continuing these experiments for several weeks, and repeating them with different narcotic solutions on himself and on the ears of rabbits, he felt he might venture, with a more convenient apparatus, to bring them more publicly forward. He obtained, therefore, a Pulvermacher chain, consisting of 120 links or elements, each element having a surface of double extent as compared with the ordinary chain. The shock given by this chain on completing the circuit is slight even when it is in full action; while the chemical effects are exceedingly good, water being decomposed freely. He first applied this voltaic pile in conjunction with a narcotic to the ear of a rabbit. The ear was first well shaved; it was very sensitive, and a little scratch during the shaving process made the creature struggle and call out lustily. He then placed the ear between two copper plates with moist sponge interposing and connected the external plate with the positive and the internal plate with the negative pole of the battery. On establishing the circuit a slight expression of pain was evinced, and during the whole time of this application the animal was uneasy, and the plates held gently together by insulated spring clips, had often to be readjusted. At the end of half an hour, on removing the plates, he found the ear slightly reddened in colour, and more sensitive to the needle point than before.

Satisfied as to the negative influence of the simple current, he now moistened the sponge connected with the plate of the positive pole, with tincture of aconite; and, moistening the opposite sponge again with water, he supplied the plates in the same way as before, and with the same pressure. In ten minutes he found that the margin of the ear would bear pricking without exciting movement; and at the end of the quarter hour, on removing the plates, the ear was as insensible as could be wished. He transfixed it in every direction, pinched it, incised it; but the animal was clearly unconscious that any auricular appendage belonged to it on the one side. The insensibility, very little lessened, was present two hours later in the day.

To the mode of producing anæsthesia thus instituted Dr. Richardson applies the term voltaic narcotism.

On January 29th of this year, he repeated the above experiment on the ear of a dog; Drs. Halford and Henry, and Messrs. Bainbridge and Gregson, being present, and lending assistance. The sponge connected with the positive pole was moistened with a drachm of tincture of aconite, to which was added one grain of the alcoholic extract; and the plates, secured with slips as before, were kept applied for thirty-five minutes. Then removed, the ear was found insensible at two points only; the plates, therefore, were once more applied with more narcotic solution, and were retained for thirty minutes. On removal, the ear was found pretty generally insensible over the parts where the plates had been applied. Transfixion could be made without the elicitation of pain; and Dr. Halford incised the ear transversely in a line nearly an inch long, without causing any expression of pain. Fifteen minutes later, the insensibility was such that the ear could be transfixed as before without exciting sensation; and at the close of twenty minutes Dr. Halford applied caustic freely to the incised wound with the same result. The animal, meanwhile, seemed exceedingly well. When his other ear was pricked he speedily let him know about it; and, for a few minutes, from having been long held in a cramped position, he walked a little unsteadily; but he took food, showed nothing in the way of coma, and recovered without any unfavorable symptoms, the wound healing excellently well.

On the 1st of February, Dr. Halford and Mr. Bainbridge being present, the left hind leg of a dog was shaved. Around the upper part of the limb was wrapped a broad copper band, including a sponge saturated with a solution made as follows: Tincture of aconite ʒij , alcoholic extract of aconite ʒj , chloroform ʒij . A third part of this solution was placed on the layer of sponge. Around the lower part of the limb, below the ankle, was wrapped another plate of copper, enclosing a sponge saturated with water. This done, Dr. Richardson connected the upper plate with the positive pole of the voltaic battery, and the lower plate with a negative pole, and at once set the battery in action. Eleven minutes after the establishment of the current, the parts included between the poles were so insensible that they could be transfixed at any point without exciting pain; and at twelve minutes Dr. Halford divided the tendo-Achillis by subcutaneous section, with the same result. The insensibility also extended for a short distance beyond the upper plate. The current was now sustained until the end of an hour, when the upper plate being removed, Dr. Halford proceeded to amputate the limb. The incision was commenced on the inner side of the leg, some little distance (three quarters of an inch) below the knee, and was carried across the tibia to the outer margin of the fibula. The limb was then transfixed, and a flap obtained from the posterior half of the limb; a circular sweep was carried to separate the muscles, and the interosseous membrane was divided. Throughout all these steps of the operation, except in the last, the animal gave no wince or indication of pain; but in dividing the interosseous membrane, he drew up the limb; and in sawing through the bones he gave a scream, as if from pain or terror. In the after-steps, including the tying of two arteries and insertion of six sutures, there was no indication of pain.

Within twenty minutes the animal had eaten two plates of meat, and

walked about on his three legs with the utmost unconcern. The wound healed well and gave an excellent stump.

Subsequently Dr. Richardson narcotized the hinder leg of another dog by the same process. The operation was done too hurriedly; the leg was badly shaved, and the plates did not fit. The current was kept up for forty minutes, but long before this time the parts included between the poles were sufficiently insensible to allow of division of the tendo-Achillis by subcutaneous section. In amputating the limb, an operation performed by Dr. Halford, pain was evinced at one point in dividing the posterior flap, and much pain in sawing through the bones. In the other steps of the operation there was no expression of suffering. Seventeen hours after the operation Dr. Halford found it necessary to insert new sutures in the flaps. The parts were still so insensible that no pain was elicited. The wound, however, healed favorably, and the animal showed no sign whatever that could be considered unfavorable.

The first operation on the human subject under this anæsthetic process was performed by Dr. Halford and Dr. Richardson, on February 3d. Dr. Halford had an infant patient ten weeks old, with a nævus about the size of a shilling on the right shoulder. The child was very irritable when it was brought to the operation, and continued troublesome; but the operation succeeded well. A ring of copper, lined beneath with moistened sponge, was made to surround the nævus, and was connected with the negative pole, while a disc of copper, which rather more than covered the nævus, and enclosed a thin layer of sponge, was adapted to the nævus itself, and was connected with the positive pole of the battery. This latter sponge, first moistened with water, was charged with narcotic solution consisting of five minims of the tincture of aconite and five of chloroform. After a quarter of an hour, during which the current was made, the narcotic sponge was removed, and the parts beneath were found to be quite white in colour. They reapplied the sponge for another quarter of an hour, with more solution, and, on removal, Dr. Richardson believed the parts to be ready for operation; but Dr. Halford, wishing to be perfectly sure, urged two more applications. He then transfixed the nævus through the base with two long needles, and ligatured the mass with all the firmness he could; but neither transfixion nor ligature gave any pain. Directly after the operation the infant fell asleep, and the recovery was as simple and quiet as could be desired.

On February 4th, Mr. Kempton, of Princes Street, Hanover Square, gave Dr. Richardson an opportunity of trying this narcotic system in a case of tooth-extraction. The tooth was the last upper left molar, and very firm. It was hollow externally, and the cavity admitted of being easily filled with a dossil of lint steeped in a narcotic solution composed of five minims each of tincture of aconite and chloroform. Mr. Kempton, after introducing the narcotic, connected a fine excavator with the positive pole of the battery, and with the point of the excavator pressed on the lint while Dr. Richardson placed the negative pole, tipped with moistened sponge, externally, immediately below the lobe of the left ear. The apparatus being imperfect for the purpose, and the application of the poles being dependent on the hands, contact was sometimes broken, and

occasional slight shocks were given, of which the patient has since complained. These were accidental, and avoidable in another case. After an application of five minutes, Dr. Richardson removed the poles, and found that a sensation of numbness had commenced in the tooth, and extended superiorly and laterally, from the tooth as a centre. A new dossil of lint, again saturated with narcotic solution, was inserted in the tooth, and the poles reapplied. In two or three minutes the sensation of shock was not felt—even when contact was intentionally broken. After seven minutes, the poles were removed, and Mr. Kempton extracted the tooth without the patient experiencing the slightest sensation. He explained that he could hear the breakage between the tooth and its socket, but without the merest trace of pain.

On Saturday, February 5th, Dr. Richardson punctured a bursa on the wrist of a girl by subcutaneous section. The plate connected with the negative pole of the battery was placed on the anterior part of the wrist, while the bursa was covered with a disc, which enclosed a sponge holding five minims of a tincture of aconite, made from the alcoholic extract, each drachm of the tincture holding five grains of the extract and five minims of chloroform. This application was three times repeated for sixteen minutes. After the second application the parts to be operated on were white, and at the time of the operation were so insensible that the patient, whose head was averted, had no consciousness at all that anything was being done. Sixteen hours later the centre part, where the narcotic had been applied, was still partly insensible. The case did well.

As a test experiment, Dr. Richardson placed on another portion of the arm of this patient a narcotic solution made in the same way as that which had been used with the voltaic current. The solution was also applied in the same way under a copper disc. On removal after half an hour the skin beneath was slightly reddened, but there was no insensibility.

Since the appearance of this paper some controversy has taken place as to the relative value of the narcotic solution and the electrical force in the production of the local insensibility; but it does not seem to us that anything that has been said invalidates the experiments given above. On several subsequent occasions Dr. Richardson has publicly explained that the local application of a narcotic solution will produce a superficial insensibility, a point admitted by him in the commencement of the above paper, and abundantly proved by the previous experiments of Simpson, Snow, and especially of Nunneley. The points to be considered are, whether, by the application of the electrical current with the narcotic solution, the insensibility is made to extend deeper, and to become more effective. It is certain that, with some failures, which are even more difficult to explain than the successes, Dr. Richardson has produced local insensibility by his process, in a degree singularly striking. And although there are practical difficulties at this moment in the way, and many anomalous phenomena requiring to be explained, the subject he has opened is one which deserves careful experimental inquiry. Not an inquiry based on predetermined opposition and sustained by churlish argument, but such a one as shall be honestly directed to the discovery of the simple question—can electricity be applied with advantage as ■

means of introducing narcotic remedies into parts locally? We rest satisfied that Dr. Richardson himself, guided by further experiment, will answer this question fairly, unbiassed by any of the criticisms to which all men of experimental mind are of necessity subjected.

On Chloroform and other Anæsthetics; their action and administration.

By JOHN SNOW, M.D., Licentiate of the Royal College of Physicians. Edited, with a Memoir of the Author, by BENJAMIN W. RICHARDSON, M.D. (London: John Churchill, New Burlington Street, 1859.)

This work, which was just completed before Dr. Snow's untimely death, furnishes us with a full account of the state of our present knowledge respecting anæsthetics. Of this the fact that Dr. Snow is the author is a sufficient guarantee. The interest of the work, moreover, is greatly enhanced by a memoir of the author from the graphic pen of his friend Dr. Richardson—a memoir which furnishes us with another instructive and encouraging instance of large success achieved from small beginnings, and without any very extraordinary talents, by the force of patience and perseverance.

There are few topics connected directly or indirectly with the subject of which it treats, on which the reader will not find all the information he can desire in the work before us; but the part which is at once most interesting and most important is that which furnishes Dr. Snow's own personal experience in the use of chloroform—an experience extending to upwards of 4000 cases—and to this part, therefore, we will chiefly confine ourselves in the present notice.

And, first, with respect to the physiological effects of chloroform. These are divided by Dr. Snow into six stages or degrees. In the first there is perfect consciousness of what is going on, and the patient experiences noise in the ears, tingling in the limbs, and, very generally, considerable diminution of sensibility. In the second stage there is no longer any correct consciousness, and the patient will often endeavour to push the inhaler away. Often, also, he will laugh or sing, or talk loudly. It does not appear to be necessary to proceed beyond this state in obstetric practice; and the patient, moreover, is generally in this condition during the greater part of the time occupied in protracted operations.

In the third degree there are no longer any voluntary motions. The eyes are turned upwards. The conjunctivæ are congested, and spasms and rigidity of the muscles occur, especially in robust males. Articulation is indistinct and unintelligible. When this state is fully developed there is complete anæsthesia, which is best evidenced by the absence of winking when the edges of the eyelids are gently touched, and the patient is then in the proper state for the performance of any surgical operation.

In the fourth degree the breathings are stertorous, the pupils dilated, and the muscles completely relaxed. There is of course perfect insensibility. It only seems requisite to proceed to this stage in the reduction of long-standing dislocations, in muscular subjects, and in very

painful operations. After this state the breathing becomes embarrassed, the heart pulsating distinctly, even after the breathing has altogether ceased. This might be termed the fifth stage. The ultimate and last effect of chloroform is to destroy the irritability of the muscles, and to produce post-mortem rigidity.

Dr. Snow agrees with Flourens in his remarks upon the action of sulphuric ether, and considers them equally applicable in the case of chloroform, namely, that, "1st. The cerebral lobes first lose their power—that of intellect. 2dly. The cerebellum loses the power of regulating locomotion. 3dly. The spinal marrow loses the principle of sensibility and of motion, the medulla oblongata still retains its function, and the animal continues to live; with loss of power in the medulla oblongata, life is lost;" but the ganglionic nerves, adds Dr. Snow, may still perform their functions, and the heart and intestines continue to move for a time, often with vigour.

As to the circumstances which influence or modify the action of chloroform, the following particulars are enumerated:

1. Age. Chloroform acts very favorably on children. Its influence subsides slowly in old persons. Dr. Snow administered it to one patient of more than ninety years of age without any ill effect.

2. Strength or debility. Those who are weak are quickly and favorably affected by it; the strong and muscular struggle much.

3. Hysteria. This is occasionally produced by the inhalation of chloroform. Care should be taken, and the quantity administered should be much diminished during the irregular breathing which often accompanies this state.

4. Epilepsy is occasionally brought on in those who have already suffered from a previous attack.

5. Pregnancy and the menstrual period do not interfere with the administration of chloroform.

6. In diseases of the lungs, especially in phthisis, chloroform appears to exert no prejudicial, but rather a soothing effect, allaying the cough.

7. In diseases of the heart, though the use of chloroform is thought to be unsafe, especially when there is fatty degeneration, the author contends that there can be no objection to it, since on several occasions he has found that there is less disturbance of the pulse when under its influence than when the patient is enduring pain.

8. Cerebral affections offer no obstacle to the administration of chloroform.

In reference to the preparations for inhaling chloroform, Dr. Snow, in common with others, suggests the avoidance of a full meal immediately before inhalation in order that vomiting may not occur. The supine, or at least the semirecumbent posture, should always be insisted on, and everything should be done which may tend to tranquillise and soothe the patient.

Various modes of administering chloroform have been adopted since its introduction. Dr. Snow objects to a cambric handkerchief on account of the difficulty of regulating the quantity, and recommends his own well-known inhaler, of which a good engraving is given.

Although the pulse of itself gives no indication as to how far a patient

is under the influence of chloroform, it is proper to pay attention to it, not only during the first administration, but also throughout the operation, especially if it be attended with much bleeding.

In elderly persons the pulse sometimes becomes intermittent or irregular, and in such cases it is well to discontinue the inhalation for a few seconds. If the precaution be taken to ensure that the air the patient breathes shall not contain more than five per cent. of the vapour, the pulse can never be seriously affected by the direct action of the chloroform on the heart, and then the state of the breathing affords the best warning against continuing the inhalation for too long a period. Wherever stertor is observed the inhalation should be suspended.

In cases where there is violent struggling Dr. Snow has always succeeded in subduing it by continuing the inhalation slowly and cautiously, taking care that the patient did not take in a deep inspiration of the undiluted vapour. The action of chloroform upon the pupils does not appear to be constant; these being sometimes dilated, sometimes contracted, and varying in some measure with the amount of light.

The average time required for the due administration of chloroform is two minutes in infants, three minutes in children, and four or five minutes in adults. The patients usually become conscious in five minutes after the discontinuance of the inhalation; children often drop off into a deep sleep that may last for some hours. A little languor usually succeeds the recovery, and it is desirable that in all cases the patient should sit or lie quietly for an hour or so, and the more so as this measure frequently prevents vomiting. If the vomiting which so often succeeds should not subside of itself, a little brandy and water should be given, and if this fail in removing it a dose of opium is usually successful. Faintness and depression rarely occur. They are usually only precursory to the vomiting, and should be treated by ordinary means.

In the section on the cause and prevention of death from the inhalation of chloroform, Dr. Snow arrives at the following conclusions:

1. That if an animal be kept for a long time under the deep influence of chloroform, it becomes ultimately exhausted, the circulation and respiration being gradually weakened, and ceasing nearly at the same time.

2. That when chloroform is given in moderate quantity, that is, where it constitutes about four per cent. of the inspired air, the respiration *first* declines, and then ceases, from the action of the chloroform on the nervous centres, after which the heart, receiving only venous blood, charged with the narcotic, also gradually fails in its action.

3. That when the dose of chloroform is considerable and rapidly given constituting ten, twelve, or more per cent., the circulation and respiration fail together, the action of the heart being arrested by the direct influence of the chloroform upon it—a true paralysis of the heart occurring.

4. As the result of several experiments upon animals with air strongly impregnated with chloroform, though this was withdrawn as soon as the heart ceased to beat and fresh air was drawn in by inspiratory efforts, it very rarely had the effect of restoring the heart's action, although this happened so frequently when that organ had ceased to act, on account of suspension of the breathing, as the effect of more diluted vapour.

It follows as the application of these conclusions that, in cases where

chloroform considerably diluted has been given slowly, death is unlikely to occur. The heart's action continues even after respiration has ceased, and there is a tendency to spontaneous recovery, which may be materially aided by artificial respiration.

On the other hand, air saturated with chloroform, that is, containing ten or twelve per cent. at 60° Fahr., or nineteen per cent. at 70°, may, if given rapidly, at once paralyse the heart: and from this condition the chances of recovery are small. Details of fifty fatal cases are given, and certain points of analogy are pointed out as existing in all of them. The death of the patient was in nearly all the instances sudden. In some cases death occurred at the very commencement of the inhalation, in others it occurred after some moderate space of time, and in a third set of cases death resulted after the vapour had been long inhaled in a very concentrated state. Many of the patients fell into a state of collapse, becoming white or livid in the face; sometimes convulsions, in other instances complete relaxation of the limbs, occurred. Though the pulse was often closely attended to, in many of the cases no premonitory symptoms occurred; it beat regularly and naturally till it stopped suddenly for ever. The breathing was noticed in some cases to be irregular, often with a "catch" or "hitch," and in those cases where the vapour had been long and fully inhaled, it became stertorous and slow.

Dr. Snow concludes that in all the cases which are fully detailed there is every reason to believe that death took place by cardiac syncope, or arrest of the action of the heart. In forty out of the fifty cases given the symptoms of danger appear to have arisen from this cause, and not from the over-action of chloroform on the brain and nervous system. It was only in four cases that the breathing appeared to be embarrassed and arrested, by the effect of the chloroform on the brain and medulla oblongata at the time when the action of the heart was arrested by it; and only in one of these cases that the breathing was distinctly arrested by the effect of the chloroform, a few seconds before that agent also arrested the action of the heart.

The post-mortem appearances which were commonly met with, consisted of a fluid state of the blood, an empty state of the cavities of the heart, with not unfrequently fatty degeneration of its muscular tissue, and some congestion of the lungs and brain, rarely considerable in degree.

The means resorted to for recovery when death was either impending or manifest, consisted in different instances in placing the patient, if previously sitting, in the prone posture, dashing cold water over the face and body; galvanism to the back and belly, to produce contraction of the diaphragm; artificial respiration, either by insufflation or by the Marshall Hall method; and lastly, the administration of stimulants, as brandy, ammonia, and the like.

With respect to the nature of the syncope which produces death, Dr. Snow is of opinion that there are two forms, one form being illustrated by the mode of death which ensues after great losses of blood, in which the heart ceases to beat from an insufficient supply of blood; and a second form, which he terms cardiac syncope, in opposition to the former or anæmic syncope, a form which he illustrated by death from narcotics or from fatty degeneration of its tissue, or from its being overpowered by the quantity of blood distending it.

The safety with which chloroform may be administered when due care and caution are exercised, may be gathered from the fact, that Dr. Snow had memoranda of 4000 cases in which he had given it, without, as he believes, the loss of a single patient. In enumerating the different operations in which chloroform has been given, it appears that there is scarcely any operation in which it has not been given, except that of cleft palate.

A very remarkable point is made out by the statistics of anæsthetics namely, that all the great amputations are much less frequently performed than they were a few years ago. This appears to be owing to the great facilities that are now given to the surgeon for making exploratory incisions, &c., while the patient is insensible, and also to the increasing frequency of the operations for incision of joints.

Chloroform should be given in parturition, according to Dr. Snow, where the patient desires it, and the time for administering it is when the *os uteri is fully dilated*, and the expulsive pains are present; yet it may be given earlier. The anæsthesia induced should never be complete, otherwise the assistance of the respiratory muscles would not take place. Only a few minims should be used at one time, and very little may be necessary, though in a protracted labour of thirty-one hours, as much as seventeen fluid ounces were given by Dr. Snow.

The inhalation may be repeated when the placenta is being thrown off. The chief medical cases in which chloroform has been used are, neuralgia, asthma, spasmodic croup, hooping-cough, infantile convulsions, delirium tremens, delirium in fever, tetanus and hysterical paralysis and contractions.

III.

REPORT ON THE PROGRESS OF MIDWIFERY.

On the treatment of Puerperal Fever. By MM. GUÉRARD, DEPAUL, BEAU, HERVEZ DE CHEGOIN, TROUSSEAU, PAUL DUBOIS, CRUVEILHIER, DANYAU, CAZEAU, JACQUEMIER, LEGROUX, BEHIER, BOUILLAUD, PIORRY, VELPEAU, J. GUERIN, DEPAUL. ('Archiv. Gén. de Med.,' April, May, June, July, August, 1858.)

ON the 23d of February, 1858, a paper having the above title was read before the Academy of Medicine in Paris by M. Guérard. Its main object was to provoke a discussion, and so well did it succeed in this, that four whole months were allowed to elapse before the matter came to an end. It will not be necessary, however, to do more than indicate some of the chief points in the argument; for, as it seems to us, the discussion, though long enough and not without sufficient briskness, did not result in anything either novel or important.

It must be premised, that the term "puerperal fever" is not made to include a certain number of phenomena which may be present in newly delivered women—milk-fever, bilious or gastric fever, or that typhoid state which is not unfrequently developed after the tenth day in consequence of certain changes in portions of retained placenta, or in uterine coagula. By the term is understood the group of phenomena which is ordinarily and regularly classed under it.

With respect to the nature of puerperal fever, MM. Guérard, Dubois, Danyau, and Depaul, are of the same mind. They regard it as an idiopathic septic disorder. They think that the epidemic prevalency, and the infectious and contagious characters, are conclusive arguments in favour of the view. A miasm penetrates into the economy, they think, and produces consecutively the rapid and multiplied formation of purulent collections; and these collections are the effects, and not the cause, of the alteration of the blood. This they hold to be proved expressly by the fact that there are numerous fatal cases without any trace of pus, and others where there is pus in the veins and lymphatics, without any

evidences of local inflammation. The miasm itself they look upon as arising in the joint influence of the locality in which the puerperal woman is placed, and of the woman herself.

The other speakers combat these statements. M. Beau is of opinion that the disorder is an inflammatory development of an inflammatory diathesis. With M. Piorry it is still an inflammation—uteritis, phlebitis, peritonitis. M. Cazeau regards it as a low inflammation, connected with some peculiar change in the blood, or brought about by some epidemic constitution of things. With M. Jacquemier the disorder is metro-peritonitis. With M. Legroux it is phlebitis, lymphatitis, metritis, metro-peritonitis. M. Béhier considers it as a purulent phlebitis. M. Bouillaud holds that puerperal fever is a septic and purulent infection of the blood, associated with an inflammatory element. M. Trousseau regards it as an inflammation of a peculiar nature, due to a specific cause, which may happen not only in puerperal women, but in the foetus, in the newly-born infant, in women at any time, and even in wounded persons of either sex or any age. M. Cruveilhier holds it to be a malady arising in miasmatic infection, and connected with a purulent condition of the lymphatics of the uterus and its dependencies. With M. Hervez de Chégoin it is sometimes purulent infection, sometimes putrid infection. With M. J. Guérin it is a putrid infection, caused by the uterus remaining in the state of an open wound, for want of the proper and natural contraction in its fibres.

With respect to treatment, there was as little agreement as with respect to pathology; but all seemed to allow that there was no one invariable mode of treatment for all cases and all times, and none which in any case deserved absolute confidence. Quinine, mercury, ipecacuanha, and other remedies—things the most dissimilar—had each their advocates, and there was no agreement as to prophylactic measures. M. Cruveilhier was for suppressing all great maternity establishments, and having arrangements made by which the poor should be confined at home, or in small establishments in very healthy places.

Practical Midwifery: comprising an account of 13,748 Deliveries which occurred in the Dublin Lying-in Hospital during a period of seven years, commencing November, 1847. By EDWARD B. SINCLAIR, A.B., T.C.D.; and GEORGE JOHNSTON, M.D. Edin. (London and Dublin; Churchill, and McGlashan and Gill, 1858, 8vo, pp. 574.)

The title of this work, 'Practical Midwifery,' scarcely expresses its purport. It is a most valuable report of the Dublin Lying-in Hospital, from the year 1847 to 1854—the period of Dr. Shekleton's superintendence.

A brief history of this great hospital is given in the introduction. It was opened by Dr. Bartholomew Moss, in 1756, and placed under the control of a master and two assistants—the former for a period of seven years, the latter for three. The master was elected from the assistants, the assistants from pupils of the hospital. The pupils were of two classes—"students in medicine or apprentices of surgeons," and "women who intend to practise midwifery." Both were under the instruction of the

master and assistants, and at the end of six months they received, after examination, a certificate of competency. A register has been kept of the pupils since the foundation of the hospital, and "a careful perusal of the records has supplied us with the following information: from the year 1786 to the close of 1854 there have been 2875 medical pupils; and, in addition, 656 women, who have attended for the term of six months; the former to learn obstetric medicine, and the latter the nurse-tending of lying-in patients. An analysis of the 2875 gentlemen who have been registered shows that there were 2608 Irish, 220 English, 14 Scotch, 4 from the British army, 8 from the colonies, 16 were Americans, 2 Germans, 1 a Swiss, and 2 were Russians. It may be interesting here to mention, that the first gentleman who crossed the waters to attend the practice of the hospital was a Mr. Cunningham, from Pennsylvania, registered on the 9th of May, 1798; and the first Englishman we find entered was a Mr. Harrison, who was registered on the 15th of August, 1838."

From the opening of the hospital to the termination of the year 1847 there have been 136,100 women delivered within its walls. These have given birth to 158,535 children; 2366 of the women delivered had twins; 33 gave birth to triplets; and but one had quadruplets. Of the 136,100 women delivered, 1903 died in childbed. Of the total number of children born, viz., 158,535, there were 82,314 males, and 76,221 females, leaving an overplus in favour of the males to the amount of 6093. Of the entire number of children born, 9291 were dead-born, 5733 died in hospital, and 143,511 went out alive.

Thus it appears that about 1 child in every $17\frac{1}{2}$ was stillborn, and 1 out of every 26 born alive died in hospital; it is also apparent that nearly 1 woman in every 66 had twins, and 1 in 4730 had triplets. The mortality of the women delivered, from all causes, was 1 in 82.

Patients were not admitted into hospital till their labour had commenced, or until there were symptoms present to indicate its approach. As soon as possible after the entrance of a patient into the labour-ward, her condition was inquired into by one of the assistants, and any treatment adopted that was immediately demanded. When she was found to be in the second stage of labour, she was undressed, and placed upon a small, low, narrow bed, having curtains, open at the top, and situated at one side of the fireplace, called "the couch," upon which, when practicable, all patients were delivered; and here she was attended *secundum artem*. She was then permitted to remain, for an hour after her delivery, on this couch; and at the expiration of that time, should there have been no contra-indication—such as a tendency to hæmorrhage, &c., &c.—she was carried horizontally to her bed, which was fresh and dry for her reception. "Primiparous women were not permitted to rise till the fifth day; otherwise, they were allowed to be dressed, and to lie outside the bed, on the fourth; and on the eighth day they were discharged, if well enough, and if, as before mentioned, they desired it." Such, however, was the *vis vitæ* of the Irish parturient, that "it was with the greatest difficulty patients could be prevailed upon to remain in hospital even so long as the eighth day; and numbers, to our own knowledge (the author's), have insisted upon being discharged before the ordinary time."

The arrangements to obviate the scourge of all such hospitals—puerperal fever—are important. The hospital contains 103 beds, which “are diffused over eleven wards, nine of which are used in their turn for the reception of labour patients, one as a convalescent room, and the other for the reception of women labouring under uterine affections, or diseases of the sexual system in general. These wards, with the exception of three, are distributed along the first and second stories of the building, the ground-floor being occupied by the officers of the institution. Through the centre of each flat runs a corridor, lighted and ventilated at each end; and on either side of these are two wards, the front ones looking into Great-Britain Street, and the rear into Rutland Square, commonly called the ‘Rotundo Gardens.’ Each ward consists of one large room, off which there are two small ones; in the former patients are delivered and retained, and the latter are found useful when it is deemed advantageous to isolate certain cases, especially those of a contagious nature.*

“Through these wards is maintained a perfect and thorough ventilation, the foul air being carried off by means of vent tubes, which open upon the roof of the building; while a fresh supply is admitted from the corridors through the doors, all of which are perforated for that purpose. The effect of this arrangement is most striking, from the nearly perfect absence of puerperal odour,† even at the time that patients are being delivered in the apartment; and to this free ventilation may, we think, in some measure be attributed the paucity of those visitations of puerperal fever, so much more frequent among the large continental lying-in hospitals, where the same system is not adopted. It is, however, a *fact*, that since this method of ventilation was had recourse to, the mortality amongst the infants has been very much lessened; for previous to its introduction the deaths averaged 1 in every $4\frac{1}{2}$ of those born alive, whereas they afterwards fell to 1 in 24; and during our period of residence a case of trismus, formerly of such frequent occurrence, was looked upon as a rarity.”

“The nine labour wards were filled in succession in the following manner: for instance, as soon as the beds of the first were occupied with recently delivered women, labour was transferred to the second, and so on; and it might be taken, as an average, that each had its complement in less than forty-eight hours—generally in twenty-four. Now, the patients, if sufficiently well, and if they desire it (for it was, and, we believe, still is, perfectly optional with them), were discharged on the eighth day from their delivery; but, under any circumstance, the ward in which they were delivered was emptied upon that day, and the patients, if not strong enough to be discharged, were removed to the convalescent room. Thus in eight days the ward first filled was empty;‡

* These small rooms are sometimes used as “private wards,” when practicable. Thus any one who may desire to be delivered, and treated privately, can have one, on paying the sum of £1 for such convenience, which sum goes to the funds of the hospital.

† This fact is also mentioned by Dr. F. H. Arneth, *op. cit.*

‡ If any case of puerperal fever, or other disease of a contagious nature, had occurred in the ward, it was at once shut up for a considerable space of time, and the necessary measures adopted for its purification. In the mean time, one of the idle wards, which were closed for want of funds, was opened for the reception of patients in place of the infected ward.

and then a thorough cleansing and scouring was put in practice, and a free draught of air permitted through it, till just before it was again to be occupied with a fresh batch of labour patients, which was generally not until it had two or three days' rest."

During the mastership of Dr. Shekleton, 13,748 women were delivered of 13,933 children, 7177 being males, 6756 females. The number of males exceeded that of the females by 421. "The total number of stillborn children, from all causes, was 968, or 1 in about every 14, and nearly one third of the total number given birth to. Of these 968 stillborn, 487 were putrid at the time of birth, which, deducted from the total number, viz., 261, leave 481, or nearly 1 in every 28 of entire births, whose death was either the result of actual labour, or took place immediately prior to its setting in. Upon analysing these latter, we find that 284 were males, and 197 were females, and 99 of the 481 were premature—namely, 43 males and 56 females.

"The proportion of women having twins can be seen, from the above statements, to have been about 1 in 59; and the proportion of women dying, from all causes, 1 in every 84 and nearly one third. In order, however, to arrive at a correct idea of the mortality of the hospital, we may be permitted to deduct from the total number of deaths those who were admitted in a dying state, which latter amounted to 17. We thus leave a balance of 146 dying from all causes, or, *quam proximè*, 1 in 94. Then, if we deduct from the above 146 fatal cases those whose deaths had arisen from visitations of puerperal epidemic—viz., 70 victims to that disease—we reduce the number to the sum of 76 dying from all causes, or 1 in $180\frac{2}{3}$." But upon examination of the 76 remaining fatal cases, we find in all 40 deaths from other than puerperal causes. If, then, we deduct these from the 76 fatal cases, there remain 36 deaths which originated from labour, or 1 in every $381\frac{1}{2}$ of those delivered.

The mortality, including that from puerperal fever and all other causes, was 1 in 84 cases; deaths from labour alone 1 in 381. This forms a most favorable contrast to the great continental hospitals, whose mortality varies from 1 in 13 to 1 in 25 cases. It was found also, that the least amount of mortality was in the months of May, the greatest those of December, the former series being in a ratio of 1 in $184\frac{5}{7}$, that of the latter 1 to $46\frac{1}{2}$. "The May series during the seven years presented the least amount of mortality, and the greatest amount of deliveries; and December the least number of deliveries, and the greatest amount of mortality."

The following is the arrangement of labours adopted in the report :

CLASS.	DIVISION.	SUBDIVISIONS.
I. NATURAL LABOUR.	1st. Purely natural. 2d. Varieties of natural.	<i>a</i> Face. <i>b</i> Face to pubes. <i>c</i> Arm with head.
II. DIFFICULT LABOUR.	1st. Tedious. 2d. Instrumental.	<i>a</i> Forceps. <i>b</i> Crotchet.
III. PRETERNATURAL LABOUR.	1st. Breech and lower extremities. 2d. Thorax and upper ditto.	
IV. ANOMALOUS LABOUR.	1st. Plurality of children. 2d. Prolapse of Funis. 3d. Hæmorrhage. 4th. Convulsions. 5th. Rupture of uterus, vagina, or both. 6th. Inversion of the uterus. 7th. Premature labour. 8th. Retained placenta. 9th. Labial thrombus. &c., &c.	<i>a</i> Twins. <i>b</i> Triplets. <i>a</i> Accidental. <i>b</i> Unavoidable. <i>a</i> Apoplectic. <i>b</i> Hysterical.

With regard to *natural labours*, the practice with regard to the support of the perinæum is important.

“ During the second stage of labour, more especially with primiparous women, great care was directed towards the support of the perinæum, but not until the head had pushed the structures beyond the nates—in other words, when a large tumour occupied that particular region. The support was only maintained during uterine action; and it was considered that the hand, without any napkin intervening between it and the parts, save just over the anus, and a little distance from its verge, was the best practice, since by this means a more equable and exact sustaining power could be rendered, and the rate of progress better estimated. The right hand was used for this purpose, after either of the following manners: Sometimes the ulnar edge of the hand was made to rest across the posterior boundary of the perinæum, and the cleft between the thumb and forefinger to correspond to the edge of the posterior fourchette. At other times, the heel of the hand was placed so as to

correspond to the posterior boundary of the perinæum, and the hollow formed by the palm and fingers, gently grasping the distended structures, made to form a continuation, in a direction forward, of the curve of the sacrum and coccyx. The support was always forward, towards the central point of the arch of the pubis. Although particular attention was paid to the following out of this practice, there happened, nevertheless, and that often, *slight* lacerations; and it was the experience of the physicians connected with the hospital that such lacerations must sometimes occur, even under the hand of the most experienced and careful accoucheur. These rents, happily, for the most part were trifling; that is, only engaging a very short distance of the structures. When slight, they were found to be of no consequence whatever, since in all cases that came under notice they healed rapidly, and did not prolong convalescence. In very severe cases of laceration, however—which were most rare, and concerning which we shall report more fully hereafter—the consequences were not of a trivial nature.

“During the time the right hand was engaged with the perinæum, and just as the head was emerging from the outlet, it was invariably the practice to place the left over the fundus of the uterus, and with it the organ, as its capacity diminished, was, in the language of the hospital, ‘followed down.’ This treatment was strongly urged for the purpose of ensuing a uniform contraction of the uterus, and thus rendering the woman less liable to either the occurrence of hemorrhage, or the retention of the placenta. In ‘following down the uterus’ with the left hand, it was considered advisable that its ulnar edge should be completely above the fundus; in other words, that the operator’s hand should not be permitted to press the uterine tumour, but rather to grasp the organ at its summit. Some of the cases of retained placenta, from irregular contraction, were thought to have originated from such mal-application. The pause which generally follows the delivery of the head of the fœtus was occupied in the usual manipulations, unnecessary to be detailed here; it may, however, be right to mention that when the funis was coiled round the neck, it was the practice to permit the shoulders to slip through the slackened loop or loops; but, when sufficiently loose, the funis was slipped over the head. In not one single instance was it found imperative to divide the cord prior to the complete birth of the infant.”

“Of the 11,874 mothers, whose deliveries had been purely natural, 67 died: 41 died of puerperal fever; * 4 of phlebitis; 3 of typhus; 2 of acute bronchitis; 1 of asthma; 1 of acute pleuritis; 4 of pneumonia; 3 of phthisis; 3 of disease of the heart; 1 of mental depression; 1 of erysipelas; 1 of scarlatina; 1 from destitution; and 1 of abscess in the brain.”

The effect of mental depression is remarkably shown in one case:

“The patient, aged 23, was admitted at full term of her second pregnancy, and delivered after a labour which lasted twelve hours—the second stage having been perfectly easy—of a living girl. For the first two days of her convalescence she progressed as favorably as possible, much more so than could have been expected, inasmuch as she was found frequently crying, and in a state of mental depression, the cause of which

* Under this term is included both peritonitis and metritis.

was a profligate and abandoned husband. On the third day she became very feverish, complained of pain in her side, and was sleepless. For this she was treated, but without effect. She did not appear to labour under any inflammatory attack of any organ whatever, and the pain she had complained of in the right side was slight, and not accompanied with any physical sign of disease; however, she gradually sank, seemingly from pure mental depression, and died on the eleventh day after delivery. An autopsy was not permitted."

Another case of sudden death after delivery is also recorded, which is deserving of attention, because of its close resemblance to two cases of sudden death that occurred in London some time ago, which were then attributed to the inhalation of chloroform; no chloroform was given in this case.

The patient was "a woman, aged 26, admitted for her first delivery in 1848. Her labour lasted twenty-three hours, the second stage having been a little tedious from inertia, to overcome which stimulating enemata were administered. Recourse was subsequently had to ergot, the enemata having proved of no avail; and the ergot was repeated in half an hour after the first dose. Twenty minutes after the ergot pains had set in, the child—a female—was born. Though the heart had been heard beating naturally previous to the exhibition of the second dose of ergot, it pulsed feebly after delivery, and respiration could not be established. This woman complained of pain in the epigastrium before her labour had commenced; and after its completion, when the binder was being applied, she referred to that region as the seat of still further uneasiness. An hour after delivery the pulse was 100; she then fell asleep, and continued so for six hours. On awaking she complained of great difficulty of breathing; her countenance had become livid and anxious; her pulse much more frequent; her belly tympanitic; and she prayed that she might be raised in bed. She was bled to twenty ounces, a sinapism was applied to the chest, and she was ordered a draught consisting of camphor and ammonia. Relief was but temporary; the dyspnoea recurred with increased severity; the extremities became cold; the pulse imperceptible; and in twelve hours after her delivery she was dead."

With regard to *face presentations*, no instance occurred in which the chin did not, during the course of labour, move forward more or less completely under the pubic arch, and become first delivered. There was no case of face presentation in which the chin moved towards the sacrum, and the vault of the head came under the pubic arch previous to delivery. When the face occupied a position behind the pubis the case was left to nature, unless delay compelled interference. Though often tried, it was not the experience of the attendants that these positions could be righted by manipulation.

In *breech presentations* it was found that in single pregnancies, when the foetal heart was heard most clearly at or a little above the umbilicus, the presentation was found to be the nates. The presence of the meconium with the discharges was not considered by any means a decisive proof of the existence of breech presentation, since it had been often observed with head presentations, and the colour given to the discharges, though like such as would have been caused by the mixture of meconium, not frequently had its origin from other sources. Few cases occurred

in which interference was unnecessary; some, however, were observed, but all happened in healthy women, who had very roomy pelvises, and who had previously given birth to more children than one. The manner of delivery was founded on the principle of leaving the case as long as possible to the natural efforts. The mode of procedure was as follows:

"The woman having been made to lie in exactly the same position as in natural labour, if the breech was found presenting in either the first or second position, the pains natural in every respect, and the pelvis roomy, no interference was deemed necessary beyond attention to the perinæum while the nates were passing through the vulva. This was guarded while the breech passed, and when the feet of the child came to touch the posterior fourchette, their too sudden egress was prevented by the hand of the attendant which was unoccupied. When the lower extremities were completely delivered, if the uterine action was sufficient, the body of the child was permitted to be extruded till past the umbilicus; but if the action was insufficient, traction was made from the hips of the child to effect that purpose, a napkin having been previously passed round the delivered parts. When the cord was under control, it was relieved of tension by drawing down a knuckle of it, and the state of its pulsations was ascertained; if they were strong, the delivery was still further permitted to proceed naturally; if, on the contrary, they were weak, had just ceased, or could not be felt at all, traction was made. When the child's thorax was delivered so far as the pubic axilla, that is, as soon as the axilla of the child could be felt behind the pubis, even supposing the labour had proceeded so far without any assistance whatever, it was then generally experienced that manual interference became necessary, the arms having changed from their flexed position across the thorax to a state of extension alongside of and above the head. If traction had been made, this change of position of the arms was certain to have taken place. In addition to the necessity of 'taking down' and delivering the arms, any delay was now dangerous, from the greater liability to pressure arresting the circulation through the placental system of the fœtus. When, then, the axilla could be touched just behind the pubis, the 'pubic arm,' as it was called, was first extracted. This arm was chosen as the first to deliver on account of its being more easily accessible than the 'sacral' one. In bringing down the pubic arm, two or more fingers of the right hand were introduced (while the body of the child was drawn a little towards the sacrum by the left hand, behind the pubis, and over the scapula; from thence they were carried over the acromion process and shoulder-joint, along the arm, down to the flexure of the elbow, and during their passage towards that point from the shoulder traction was being made in a direction from above downwards, and then towards the outlet, sweeping the arm across the chest, the delivery of the extremity having been completed from the forearm. Having freed and delivered the pubic arm, the body of the child was taken in the right hand, and it was directed forwards towards the pubis, making the pubic arch a fulcrum, so as to bring the sacral shoulder nearer to the outlet; and the body was retained in that position during the manipulation of the posterior extremity, in order that room might be afforded for the fingers of the left hand to reach the sacral scapula, and to bring down the posterior or sacral arm in a similar manner to the

pubic one. When the arms had been delivered, traction was again made from the body of the child, so as to bring the base of the cranium as low as possible into the pelvis. At this period of the delivery no time whatever was to be lost. The traction necessary to bring the head into the pelvis, as a matter of course, changed its position from its original state of flexion of the chin upon the top of the sternum to one of extension of the chin from that part, or, in other words, it changed the opposing diameter of the head from occipito-bregmatic, one of the shortest of its long diameters, to occipito-mental, the longest of those diameters, and thus further delivery was arrested, from locking of the head, till the malposition was rectified. This was accomplished in the following manner : with one or two fingers of the right hand against the occipital bone, the child's occiput was pressed upwards, which caused the chin to descend lower in the pelvis, and brought the mouth within reach, into which was then placed the forefinger of the left hand, when the occiput was still further raised with the right, and the chin brought down to the top of the sternum, in which position it was retained, and its occipito-bregmatic diameter thus made the opposing one. With the right hand *only*, and from the shoulders, traction was now again made in the direction of the axis of the outlet. The head's position was steadily maintained till completely delivered.

"Great speed, especially in first cases, was not desired during the progress of the nates through the pelvic space, since the slow and natural dilatation of that passage by the breech rendered the subsequent delivery of the head more easy and safe; besides which, the cord was considered in no danger so long as it was protected by means of the lower extremities flexed against the abdomen."

In *premature cases of breech presentation*, little assistance was necessary. "When manipulation was required, however, in these cases, the upper extremities of the fœtus were left to themselves, even when extended beyond or behind the head; for if removed from that position and delivered in the ordinary way, experience proved that contraction of the fibres of the cervix and os uteri was almost certain to take place, so as to incarcerate the head in the uterus, and thus strangle the umbilical cord against the neck of the child. This accident occurred more than once, and no efforts could free the fœtus from the grasp of the cervix."

The instrument invented by Mr. Robertson to establish respiration before the birth of the child had been tried by Dr. Shekleton in one instance, and found of decided utility; "but for it," it is said, "we should have lost the child." With regard to *foot presentations*, the point most relied on as marking the presence of a foot at the os uteri, was the even bead-like line having an inclination in some direction; this with the fact that the toes cannot be doubled up like the fingers, will serve to guide the practitioner in doubtful cases.

"The first and chiefest point in the treatment of footling presentations was to keep the membranes entire as long as possible, in order that the bag of waters might dilate the os uteri to a good extent before the extremities came into the pelvis; the woman was, therefore, maintained in a horizontal position. In all footling cases, especially with primiparous, when the membranes were ruptured very early

(which was often the case), the risk to the child was considered to have been increased."

Knee presentations were very rare; there were only seven altogether, and they of course became footlings.

Presentations of the superior extremities "required to be early discovered—this was the first indication in their treatment; the next, if they admitted of it, to turn and deliver at the proper time. When it was impossible to turn, evisceration was the only resource. Of the diagnosis in these cases something has been already said. The shoulder was distinguished from the knee by the axilla and ribs. It mattered not in the treatment whether it were shoulder or elbow. The elbow was distinguished from the knee by its smaller size, its projecting olecranon and sharper condyles. The hand from the foot, not only from the absence of the even and inclined beaded line, but also from the presence of the deeper clefts between the digits and the great divarication of the thumb from the first finger. In addition to these marks of distinction, the hand admitted of being flexed."

Taking the whole of these cases collectively, it was necessary to complete delivery in but one instance by perforating the head and extracting with the crotchet, a tolerable proof that the rickety pelvis is not frequently met with in the Dublin hospital.

Difficult labours are divided into tedious and instrumental: by tedious is understood those in which the child is delivered, after twenty-four hours, without any assistance. The delay was generally, but not always, found to occur in the first stage, and it was considered that a lengthened first stage had always a tendency to render the second also tedious. There was not therefore that immunity from danger attached to it by Dr. Churchill, and others. In enumerating the different states of rigidity of the os uteri causing a tedious labour, the authors state that—

"We never met a single case of what might be called a cartilaginous os uteri, or of disease of the uterine cervical tissue, which rendered dilatation impossible."

In strong masculine country women a form of rigidity was met with which might be predicted from their appearance, even before an examination was made. The os uteri presented to the touch either a very thin, but rounded margin, firm, tense, and wiry, or a hard thick edge—more frequently the former.

This hard thick edge has been sometimes described as cartilaginous, and must not therefore be mistaken for that diseased condition of the os uteri described by the same term, which the authors have never met with.

Another form of rigidity, which they call "undilatable," was induced by too early rupture of the membranes, by which the expanded cervix was compressed between the presentation and the brim of the pelvis; œdema was the result, and "the œdematous sensation could not be mistaken, and the os was often painful when touched. The first condition was treated by venesection and nauseating doses of tartar emetic, either separately or conjointly; but the tartar emetic was generally found sufficient. The full, plethoric, and masculine country female occasionally required both. The tartar emetic was given in a solution of the strength of half a grain to the ounce—an ounce for the first, and perhaps for the

second dose; and after vomiting had been induced, the nausea was kept up by half-ounce doses at intervals of an hour. This mode of treatment acted in a most remarkable manner upon the truly rigid os, when steadily persevered in. As soon as the doses of the solution were discontinued, the nausea quickly subsided. That form of rigidity, or 'leathery os,' induced by the early rupture of the membranes and subsequent pressure, rarely required active treatment. The horizontal position, and tepid-water rectal enemata were had recourse to; the latter were found most useful, and seemed to soothe the parts, while at the same time they acted as stimulants to the uterus, which in these cases had generally been rendered more or less inefficient from the state of the tissue of its cervix."

Extreme nervous irritability rendered the dilating pains quite inefficient, and induced a feverish state of the system.

"In the treatment of such cases two agents were made use of, viz., chloroform and opium; latterly the former was the favorite remedy. The inhalation was carried only so far as to soothe the patient—not often to complete anæsthesia; sometimes, however, when an hour or two of complete rest was deemed necessary, it was persevered in so far. After the exhibition of chloroform to complete anæsthesia in this stage of labour, and given for the reason above mentioned, we have known patients sleep, apparently perfectly naturally, for a considerable period. Inertia would sometimes set in, after a sleepless night, or some weary hours of teasing, inefficient pains, and the patient, who had been previously calm and cheerful, would become irritable and impatient; in such cases a full dose of opium, repeated if necessary, was the practice. The opium generally put a stop to the uterine action, when sleep immediately ensued, and that state was maintained by the further effects of the drug. On awakening, in such cases, the labour was completed happily without further annoyance."

Rigidity of the soft parts within the pelvis was not often observed, but sometimes "an extremely muscular condition of the interior of the pelvis, rendering the passage of the head slow, from the unyielding nature of its tissues, was often found in healthy patients, who entered for their first labours at or a little over the age of thirty. Enemata were useful in such cases also; but an extreme muscularity was frequently connected with a peculiar state of pelvis termed 'masculine.' We shall recur to the subject hereafter, when, at the same time, we shall speak of disproportion and malposition, &c."

Ergot of rye was not generally used to excite the action of the uterus, but was administered in that form of inertia where it appeared that all that was required to complete the delivery was strong action. It was given while the head was within reach of the forceps; so that at any time, should the foetal heart have become affected, the child could be saved by that instrument.

In *instrumental deliveries* the vectis was never had recourse to; the forceps were always preferred and the circumstances which rendered their application necessary were—absolute or impending danger to the mother's life, risk of injury to the mother structure, and the threatened failure of the foetal circulation. No fixed rule for their application was acted upon, because the conditions varied so much. No two cases were alike.

"Nevertheless, it might be said that one distinct maxim bearing on the use of this instrument did exist, and was always acted on, during the greater part of the period of which we write; it was this, in doubtful cases, where their application was easy, the error was on the right side to complete delivery by their aid."

If symptoms of inflammation supervened it was considered too late to apply the forceps; at the same time it was believed that any sloughing of the soft parts which might follow was rather owing to the long-continued pressure of the head than to the instrument.

"If inertia occurred in the second stage of labour, the head having entered the brim of the pelvis, and should common measures have failed to excite the uterus to more efficient contraction, ergot of rye was had recourse to. Previously, however, to its exhibition, the state of the foetal circulation was investigated by means of careful auscultation."

During the intervals of the ergotic pains, the action of the foetal heart was carefully observed, and if found to fail, the forceps were at once applied. Some cases were met where the head of the child was preternaturally developed and highly ossified, causing disproportion, and if the pressure on the passages were long continued, not a little danger.

"In such cases the presentation felt abnormally hard and unyielding, and to the experienced touch also conveyed a sensation of preternatural size. When the secondary scalp-tumour, which soon formed in these cases, did not preclude the investigation, the bones of the sutures were found to be scarcely at all overlapped, the posterior fontanelle imperceptible, and the anterior, when within reach, very small; while at the same time the finger could with difficulty, if at all, be made to insinuate itself between the presentation and the pelvis. That peculiar olive-coloured discharge, having a disagreeable odour, was found always present, more or less, in these cases."

Under the circumstances above enumerated, should symptoms of impending rupture have shown themselves, the forceps were at once attempted. Or should the pulse have risen above 100, and the tongue have become dry, these signs alone were considered sufficient to demand interference, and the progress of the head became a matter of no account.

The general deformities which sometimes admitted of the forceps were slight degrees of the ovate pelvis, but more frequently that description of pelvis termed masculine, a variety, we believe, first pointed out by Murphy as a cause of difficult labour. This pelvis had nearly always combined with it unyielding soft parts from great muscularity and a foetal head in a preternaturally advanced state of ossification. In these operations, as a general rule, the forceps were used while the patient was in a complete anaesthesia from chloroform; sometimes it was deemed advisable to dispense with that agent, but any attempt to put them on while the woman was but partially under the influence of the drug was difficult and dangerous.

"It was by no means considered necessary to feel an ear before the forceps were resorted to; indeed, it was seldom ever sought for unless to assist in determining the position. It frequently could not have been reached, even when searched for, since this instrument was very often used when the head was not half-way through the pelvis, in fact some-

times when it could have been said that the latter had scarcely more than well entered the brim."

"Out of the 13,748 women delivered in the hospital, there were, irrespective of twin cases, 200* whose deliveries were effected by the forceps. Of the children so born, 118 were male, and 82 female; 17 of the former were stillborn, 2 of them having been putrid, and 12 of the latter were born dead. Of the 200 subjects of forceps delivery, 11 died."

Craniotomy was had recourse to in pelvic deformities, impaction of the head, cicatrices of the vagina, great rigidity of the soft parts not yielding to treatment, convulsions, and other accidental complications.

"The pelvic abnormalities met with during the seven years may be classified into partial and complete.

"The partial consisted of abnormal approximation of the pillars of the pubic arch, and of the tuberosities of the ischia; preternatural development of one or both ischiatic spines; flattening of the sacrum, the diminution of the arc of its hollow, contortion forwards of its lower third, and consolidation of its coccygeal articulation.

"The complete deformities consisted of the masculine pelvis; various degrees of the ovate, or that said to be produced by rachitis; and the oblique pelvis of Naegele.

"Of that species of abnormal pelvis called cordiform, which is said to result from mollities ossium, we had no example during the period of this report.

"In addition to the deformities above enumerated, we may mention the encroachment upon the pelvic space by fibrous growths and exostoses."

As a general rule, "in all cases where the deformity was decided, that is, where the sacro-vertebral angle could be readily reached, the second stage was not suffered to proceed further; if after a shorter period natural pains made no impression upon the presentation; and when, under these circumstances, it had been ascertained that the woman had been previously delivered by means of the crotchet, craniotomy was resorted to as soon as possible.

"On the other hand, should the diminution not have been so appreciable, and the head, having entered the pelvis, become more or less impacted, so as to demand interference of some kind, the practice was, to try whether the forceps could be introduced; and if the attempt to adjust them, in every position, failed, the operation of lessening the head was undertaken without hesitation. The same course was adopted, attempts at extraction having proved unsuccessful, after they had been adjusted."

There were 130 cases of craniotomy; 104 mothers recovered; 26 died, about 1 in 5 cases.

The practice of Dr. Shekleton in this hospital seems quite opposed to that of Dr. Collins, each superintending the hospital for the same period—seven years. Dr. Collins reported the results of 16,654 cases; Dr. Shekleton of 13,748. Notwithstanding this difference in the aggregate,

* There were 26 forceps cases under the head of twins; and in three instances both children were thus extracted.

Dr. Shekleton had 577 cases protracted beyond 24 hours, a larger number than Dr. Collins, who had 430. Dr. Shekleton performed 330 operations, Dr. Collins only 100; the former used the forceps 200 times, the latter 24. Dr. Collins allowed 324 women, more than three fourths, to be delivered without aid; Dr. Shekleton only 247, less than the half. This remarkable contrast in the practice of the same hospital is interesting, if it can determine those very difficult questions on the use of instruments which have so long been the subjects of controversy; but we confess to some disappointment in this respect, as a careful analysis of both reports present, so far as the safety of mother and child are concerned, very similar results. We find that in Dr. Shekleton's 577 cases 42 mothers died, or about 1 in $13\frac{1}{2}$. In Dr. Collins's 430 cases 44 died, or about 1 in 10. The former lost 208 children, or 1 in about $2\frac{1}{2}$; the latter 148, or 1 in 3. This difference is in favour of Dr. Shekleton's practice, but not so decidedly as to prove its superiority. With regard to subsequent injury to the passages, Dr. Collins's report is rather obscure; there is, however, one case of vesico-vaginal fistula,* and about five of sloughing in the vagina, but two of them died of puerperal fever. Dr. Shekleton gives four cases of vesico-vaginal fistula, and twenty of sloughing in the passages. The fact that erysipelas, the attendant of puerperal fever, frequently appeared in this hospital, renders it difficult to distinguish cases of slough from this poison, and that from pressure, whether of the hand or by instruments.

Plural births occurred in 233 cases, and in the management of them—

"After the birth of the first child the mode of proceeding was considered of great importance. The binder was never adjusted till after the conclusion of the placental change; but during the interval between the births of the first and second child, the uterus was commanded with the hand placed over the fundus, and the abdominal parietes were maintained pressed against that organ by its means. The prime object in treating these cases was, to have as short an interval as possible. A few minutes, therefore, having been permitted to elapse, in order that the patient might recover from the shock of the first delivery, the second membranes were punctured, and the fundus uteri gently rubbed. Generally speaking, uterine action soon set in, and the second child was quickly expelled.

"The uterus sometimes became inert after the birth of the first child, and ordinary measures failed to induce it to resume its contractions; under these circumstances ergot was unhesitatingly exhibited, and if that proved unavailing, the forceps, when the head was within reach, was had recourse to; but if, on the contrary, it was above the brim, version was performed."

There was one case of spontaneous evolution, the delivery being accomplished exactly in the manner described by the late Dr. Douglas, the breech being first completely expelled. One woman gave birth to three; the first weighed 4 lbs., the second $4\frac{1}{2}$, and the third $2\frac{3}{4}$; they were all girls; the first lived only a few minutes, the third died the morning after birth, the second survived to the seventh day.

Prolapse of the funis has been met with in ninety-eight instances.

* Delivered by crotchet after eight hours labour. Collins, p. 358.

The authors do not doubt that, independent of the preternatural length of the cord and the position of the placenta in utero, the sudden gush of the liquor amnii was the most frequent cause of prolapse of the funis.

"Most of the various methods of managing the prolapsed funis, as laid down by authors—such as returning it with the hand and hooking it upon some part of the fœtus in utero; pushing it upwards with nooses attached to catheters, sounds, &c.; enclosing it in a bag and passing it beyond the presentation by means of sponge-tents—were never had recourse to."

When this accident was recognised, the patient was kept in the horizontal position, and not permitted to exert herself, in order that the os uteri might become sufficiently extended previous to the rupture of the membranes so as to admit of turning the child.

"Should this accident have occurred with the discharge of the waters at the very commencement of the second stage, the head having been still above or only just at the brim, version was at once performed; but when the head had descended into the pelvis, or when the coil was first perceived after the presentation had entered the cavity, then there were two indications of treatment, either to sustain the cord with the fingers above the sphere of pressure, or to deliver with the forceps.

"When the funis was pulsating strongly at this particular stage of labour, elevation of the cord was first had recourse to, and if it failed, the forceps were attempted; but if, on the contrary, the pulsations were feeble, or had just ceased when the state of the case was discovered, no time was lost in their application.

"The mode of sustaining the cord above the point of pressure with the fingers has been frequently described; however, we may here state, that it was first drawn over to that region of the pelvis which afforded the greatest command over it, viz., towards the symphysis pubis. In this situation the fingers could, on account of the shallowness of the pelvis, reach above the brim. The cord was then pushed upwards, knuckle after knuckle, till the entire was in the desired position, using one finger after the other, and sustaining it with all four. This manipulation was performed during the interval of a pain, and the support was maintained during uterine action, in the hope that the head would, after passing lower into the pelvis, fill the space sufficiently to prevent the funis from again descending.

"This operation, if it may be so termed, frequently ended in failure; but sometimes was of use, even though unsuccessful, inasmuch as it has been known to ward off a fatal event to the fœtus till such time as the head could be seized in the forceps."

Two rules always served to guide the treatment of these cases: 1st, The hand once in the cavity of the uterus, it was as well to complete the delivery by version. 2d, Always to stand by with the forceps and use them as soon as practicable. A table is given from which it appears "that prolapse of the funis occurred in 98 cases, of which 31 were primiparæ; and 4 instances of this accident happened in twins, 1 of which was a primipara. Of the gross number of children given birth to, 44 were born alive, and 2 of the dead-born were also putrid. There were 66 of the children male; 34 of them born alive, and 1 of the

dead boys was also putrid. Of the 32 females, 10 were born alive, and 1 of the dead girls was putrid. Of the 32 stillborn male children, in 11 instances the funis was pulseless on admission; and of the 22 stillborn females, with 10 the pulsation of the funis was extinct on admission. In 79 cases the child presented with the head, in 5 with the upper extremity, and 14 with the breech, or lower extremities."

Accidental hæmorrhage occurred in 81 instances; in 4 of which the mothers died. 27 children were stillborn, 8 being putrid.

"The diagnosis of accidental hæmorrhage was always easy, even independent of the previous history of the case; for besides the constant oozing during the interval of the pains, and the immediate arrest of the flow upon their accession, examination readily determined the absence of the placenta from the neighbourhood of the os uteri. That peculiar sensation of elasticity conveyed to the touch when the cervix uteri was distended by the liquor amnii, contrasted strongly with the soft, doughy feel, experienced when the placenta was attached to a greater or less extent over its surface.

"The great indication in these cases was to rupture the membranes as soon as possible; and if the os uteri was not sufficiently dilated to admit of this being accomplished with facility, ergot was given, and repeated when necessary, until the orifice had attained a sufficient degree of extension to admit of the introduction of a large-sized, gum-elastic male catheter without the least risk of injury to the uterus or its contents. When the woman entered with the os uteri dilated to a considerable degree, the membranes were ruptured at once with the finger-nail.

"When the bleeding continued after the commencement of the second stage, and the head had entered the pelvic cavity, the forceps were applied as soon as practicable; but if the head had only just entered the brim, or was completely above it, version was performed; and the latter operation was adopted when, during the first stage, delivery was peremptorily demanded.

"The knowledge of the child's death, or imminent danger to the mother's life, when no other means were available, called for the use of the perforator."

Unavoidable hæmorrhage occurred in 24 instances, 8 being complete presentations of the placenta and 16 partial. Of the latter but one mother was lost. Of the former five, but "few cases came into the hospital prior to the commencement of labour, and nearly one half of those admitted entered either in a state of extreme exhaustion, or quite blanched from previous flooding." This will explain the mortality.

"In the partial variety, as in accidental hæmorrhage, the membranes were punctured, and the labour, if possible, permitted to proceed naturally. But if in these cases, the os uteri was not sufficiently dilated to admit of this being done at once, the tampon was introduced, and retained until the necessary expansion was established. This treatment may be said to have been always satisfactory. In the complete variety, where the os uteri was patulous, the tampon was at once introduced, and retained until the os was of sufficient size to admit of the easy introduction of the hand, for the purpose of performing the operation of version. In introducing the hand, the fingers, most frequently those of the left, were

made to form a cone after they had passed the vulva, and pushed gently and gradually through the os in the direction of its axis, or sometimes one finger after the other, until the whole hand had entered; but in whatever manner introduced, the hand was made to pass at one side of the placenta, after which the membranes were punctured, and the operation proceeded with. Should the feet have been found presenting, they were laid hold of, brought down at one side of the placenta, and traction having been made on them until the body of the fœtus was encircled by the os, the case was then left as much to nature as possible.

"In two cases the placenta was detached completely, in one of which it was removed altogether, and the other left *in situ*; but this practice (recommended by Professor Simpson) did not get a sufficiently fair trial to admit of any decided opinion being given as to its propriety."

Convulsions are divided into apoplectic and hysterical. The latter being extremely rare during a subsequent labour.

"The women most frequently affected were primiparæ, but 14 out of the 63 subjects of this complexity having been previously confined. The general experience as to the rarity of preternatural presentations in these labours was verified; for out of the entire number (63) there were but 5 such, and of this number 3 were in plural births."

In nearly all these cases anasarca and albuminous urine were present to a greater or less degree; nevertheless convulsions sometimes occurred, and those of the most violent forms, when neither anasarca nor albumen could be discovered.

"The treatment during an attack of convulsions consisted in, first, preventing the patient from injuring herself; and the narrow couch upon which all women were delivered, when practicable, afforded great facility for so doing, since the attendants could completely surround and protect her. To prevent the tongue from being lacerated, a gag was made of the handle of an iron spoon thickly padded with lint, which was placed between the teeth.

"As soon as the paroxysm had subsided, blood was taken from the arm very freely, and medicines given to act briskly on the bowels. The favorite purgative was calomel and jalap, formed into a bolus, five grains of the former and ten of the latter. This was not trusted to alone. A strong enema, containing castor-oil, turpentine, and assafœtida, was early administered, and repeated if necessary. Should the attacks not have diminished in intensity, the head was shaved, cold applied to the scalp, and occasionally, at the same time, flannels wrung out of hot water were wrapped round the lower extremities. Tartar-emetic solution, opium, and calomel, were also had recourse to. Opium was considered useful after free purging and bleeding. As to the exhibition of chloroform, nothing satisfactory could be said; it was, however, seldom given, and never unless depletion had been previously practised.

"As to the question of delivery, if the head was within reach of the forceps, it was at once seized, and the child extracted; but when the severity of the case demanded it, the perforator was had recourse to without hesitation.

There were 63 deliveries in which convulsions took place, 49 primiparæ, 41 multiparæ. Of these 24 were delivered by the forceps, 7 by crani-

otomy, 2 by version, 30 by natural efforts; 13 mothers died, and 25 children.

Of ruptured uterus there were 17 instances, and only 1 recovered.

"The symptoms of impending rupture, as laid down in class-books, were not always observed; in one instance the event occurred without any premonitory symptom whatever.

"Vomiting was always considered a suspicious symptom when it set in during the second stage of labour, especially if that stage had been severe or prolonged. When pain, fixed and increased on pressure, was referred to the region over the pubis, during the expulsive stage, that stage having been previously severe or protracted, the indication was to deliver as soon as possible by the means most suited to the particular case. The existence of both the symptoms together rendered the necessity of delivery more urgent.

"The symptoms of the actual occurrence of the accident were more constantly those that were usually observed, but the collapse varied much in degree, and the sensation of 'something having given way' was not always experienced."

All the women, except one, were pluriparae. Under this head is given the details of a very remarkable case (No. 6), in which, in consequence of an immense pelvic tumour, a most difficult but successful effort was made to extract the child by craniotomy; the woman died in ten minutes of ruptured uterus. There was only one instance of *inversion of the uterus*, which was reduced, the placenta being first detached. The detachment was accomplished without the least difficulty, and was not attended with hæmorrhage.

The authors' remarks on *retained placenta* are of practical importance. Generally speaking, the placenta was expelled before a quarter of an hour had elapsed. When, however, a considerable period had passed, after the lapse of half an hour, and the ordinary means failed to expel it, it was considered a case of retention. The rule of sustaining the contraction of the uterus during the expulsion of the child was always observed, and while the pupil was attending to the safety of the child, the care of the uterus was given to the midwife.

"As soon as the attendant had divided the cord, and afforded to the infant all the attention it immediately required, the charge of the uterus, which he had previously committed to the midwife, he resumed; and by the hand then gently grasping the fundus uteri, a pretty correct estimate could be formed of the subsequent efforts of that organ to discharge the placenta, and the probable result of those efforts.

"If the uterus felt spongy and large, friction was made over its fundus, and a napkin wrung out of cold water was applied to the sacrum or hypogastrium, or perhaps to both. This generally succeeded in causing this organ to contract.

"In order to discover if the after-birth had been sufficiently expelled from the uterus to admit of its steady removal, the 'contraction,' as it was called, was again intrusted to the care of the midwife, and the cord having been put on the stretch by the left hand, was made a guide for the first two fingers of the right, which was caused to pass along it, to find, if possible, the point where it joined the placenta. If this were found (unless the placenta was of the battledore variety), it was con

sidered that it had nearly, if not completely, cleared the uterus; and it was readily removed by hooking the fingers into the rugosities formed by the radiation of the umbilical vessels. Occasionally, though the cord did not lead the fingers to its insertion, yet there was enough of the placenta found without the os to admit of its easy removal.

"Removing the placenta, when partially extruded from the uterus, by means of the two fingers of one hand hooked upon its tissue, and at the same time by applying gentle pressure over the fundus uteri with the other, was preferred to pulling at the cord for that purpose, inasmuch as the latter procedure was considered likely to cause irregular contraction, by irritating the fibres of the cervix.

"When the placenta was retained by irregular contraction of the uterus, the irregular form of that organ could be sometimes felt through the abdominal parietes. Frequently, the real nature of the case was not discovered till after the hand had been introduced. The partial contraction might involve one of the angles of the uterus, the fibres of the body, those of the cervix, or those merely round the orifice. It has been known to involve the entire organ, and contract it into the form of a cylinder. It mattered little of what variety the contraction was, so far as regarded the treatment; they all required to be overcome by the hand. That form in which the fibres round the uterus were alone engaged was considered to have arisen from undue traction having been made from the cord; the introduction of the fingers, one after the other, sufficed to overcome it."

With regard to *morbid adhesions*, it was observed that a woman who had been the subject of them at a former confinement was almost certain to be again imperilled by them.

"A suspicion that the placenta was morbidly adherent always arose when a fixed pain, augmented on pressure, had been complained of, over the region of its attachment, during the latter months of gestation."

Before the seventh month of gestation the hand *was never passed in utero*. At an earlier period, if the fingers could not remove the retained after-birth, the binder was adjusted, and small doses of ergot given. There were only 26 cases of *hæmorrhage during the third stage of labour*, and of these only two died, one of phlebitis, the other of peritonitis. These facts illustrate the value of the practice adopted for its prevention. The hand of the attendant, firmly pressing on the fundus of the uterus, maintained its state of contraction until the placenta was expelled; the hand was not withdrawn, nor the binder applied, until the uterine contraction seemed to be permanent. Those women who had had floodings after delivery in a former labour, were given a dose of the infusion of ergot as soon as the head was pressing. When the child was born, the placenta expelled, and the uterus firmly contracted, the binder was then strongly applied. This generally succeeded, but, if any draining continued, the same practice was resumed.

"In 4 of these cases the bleeding continued after the removal of the placenta; in 16 cases the hand was introduced for its removal; in 10 of these it was morbidly adherent, and in 2 it was retained by irregular contractions."

Hæmorrhage soon after the expulsion of the placenta occurred in

33 instances; 2 mothers died; 1 of phlebitis, the other of adherent placenta.

It has been observed "that brisk hæmorrhage has been known to have occurred after a delivery during intoxication; and that there was always a tendency to draining with those women who partook of whiskey during labour, just before their admission, even though they did not drink to excess. The habit, we are sorry to say, was very frequent, and would have been doubtless invariably followed by flooding, had not the attendants been always on the alert.

"The relaxation of the uterus, however, which gave rise to hæmorrhage soon after complete delivery, was sometimes induced by the retention of a portion of the secundines within its cavity; at other times, a state of inertia supervened upon too rapid delivery. Occasionally there was a predisposition to hæmorrhage. And flooding also set in, though rarely, without assignable cause. The treatment pursued in these cases was, at first, similar to that employed when hæmorrhage was present during the third stage, viz., the frequently repeated application of napkins wrung out of cold water to the loins, vulva, and hypogastrium, together with friction over the fundus uteri. In the event of these means proving inefficacious, an enema of cold water was thrown up the rectum, and then a stream of water was sent along the vagina into the uterus, by means of a gum-elastic syringe, with a flexible tube, bone nozzle, and vaginal shield. This latter procedure was continued till the water came away from the vulva quite free from tinge, which was the case generally in a little better than a minute, when the uterus was invariably found firmly contracted. Nearly all the cases contained in this chapter were thus treated. After the vagino-uterine injection, the 'contraction' was generally permanent, and all further danger from hæmorrhage over; though cases did occur in which the flooding again took place, and then nothing remained but the introduction of the hand, when, generally speaking, a portion of the secundines were found in utero. The uterus never relaxed after the hand had been introduced, provided it had been perfectly emptied of its contents." There were but 5 cases of secondary hæmorrhage.

"In 2 instances the bleeding took place on the fourth day; in 1, on the eighth; in 1, on the ninth; and in 1, so late as the twenty-first day. All these women were naturally delivered; but 1 was tedious, 1 was preternatural, a breech presentation, and the rest were easy and short labours.

"In the tedious case, the hæmorrhage set in during the separation of sloughs; and in the preternatural case, it arose from the rupture of a uterine thrombus. Extreme delicacy may have occasioned the discharge in 1 instance, and in 2 it was brought on by mental excitement.

"All the children of these women were born alive save 2, viz., that of the tedious case, and the preternatural labour; in which latter the fœtus was putrid. The mothers of the two last-mentioned children died."

Some very interesting cases of *the induction of premature labour* are recorded, illustrating Kiwisch's method of induction by water-douche. The comparative merits of the instruments recommended for this purpose were put to the test, and that contrived by Dr. Sinclair, a double-cylinder syringe, was found the most efficient.

Version was performed forty-seven times. The inhalation of chloroform-vapour to complete anæsthesia, previous to attempting this operation, was always adopted. It obviated the necessity of resorting to those agents formerly in vogue for quieting the uterus previous to turning, such as opium, tartar emetic, and venesection. As a rule, it was the favorite practice to bring down one foot, and change the presentation to half a breech; thus, it was presumed, rendering the subsequent steps of the delivery less dangerous to the child.

Puerperal mania occurred in 26 cases; 18 recovered; 5 removed; 3 died, 1 of low fever, 1 of abscess of the brain, 1 of pneumonia.

"It presented itself under two varieties, the one marked by a state of great excitement, with quick pulse and flushed face; the other, and the most frequent form, of a low type.

"In the treatment of these cases restraint was never had recourse to unless absolutely demanded, and was never required save in the first variety of the disease. Careful and constant watching generally sufficed. In no instance was the lancet used after mania had set in; but some cases of convulsion during labour had been bled previous to the attack. With the exception of these, a case of placenta previa, and one of accidental hæmorrhage, there was no example of extraordinary loss of blood amongst the women affected.

"The line of treatment pursued in the first variety of mania was to clear the primæ viæ thoroughly, and at once. To effect this, a brisk emetic was given, which was followed soon afterwards by a purgative enema containing assafœtida, and cold was applied to the head. Should the excitement have continued, nausea was kept up till it was lowered, and then a sedative was occasionally given, such as hyoscyamus. In the low form of mania the bowels were cleared out with enemata, mild nourishment was freely given, and the preparations of morphia as freely used."

Ergot of rye had been used in 109 cases, and of these 92 children were saved, 9 died in hospital, and 12 were stillborn; the danger to the child, therefore, is not proved, but we must recollect the extreme care with which the foetal heart was watched during its action, and the promptitude of delivery when it was failing.

Chloroform was used with caution.

"The conclusion arrived at was, that its tendency in the greater number of instances, where complete anæsthesia had been induced, was to lessen the effect of, and prolong the intervals between, the pains. Subsequently its indiscriminate use was not deemed advisable. In natural labour it was only pressed so far as to lull pain when the woman became impatient, and the uterine action was hacking and inefficient. To the same extent only was it pressed when the throes were pronounced unbearable, or when there existed an extreme state of nervous irritability. In the generality of the above cases, however, in which it had been previously but partially used when the head began to pass the vulva, almost complete anæsthesia was produced."

Complete insensibility was produced in all cases before operation, and in none so beneficially as in those of version. With respect to the utility of chloroform inhalation during puerperal convulsions, nothing was decided, but this remarkable fact deserves attention, that "During seven years

not a single accident took place that could be attributed to the use of chloroform." The inhalation was first conducted by means of a cambric handkerchief; afterwards Dr. Fleming's inhaler was used. As in natural labour it was only pressed so far as to lull pain without disturbing consciousness, we are surprised the inhalation by the mouth, as recommended by Dr. Murphy, had not been tried. We know it to be much safer for such a purpose than those methods which promptly induce complete anæsthesia. Chloroform was administered to complete anæsthesia at various periods during the course of labour to its termination in 313 cases, primiparæ 181, pluriparæ 113, and 19 twin cases.

Puerperal fever was observed in the hospital during the whole period of seven years, but much more in a sporadic than an epidemic form. "During the years 1848, 1849, 1850, the number of puerperal cases for each year respectively were 43, 29, 15. In 1851, 1852, 1853, 1854, the numbers were 13, 3, 8, 11. Such a control of this frightful scourge can only be attributed to the extreme care with which prophylactic measures were carried out. There were altogether 129 cases, and of these 75 died. The general character of the attack was a low form of peritonitis, generally speaking requiring nutriment throughout, and, very early in the disease, stimulants. In the beginning the lancet was used, but very soon thrown aside; not so with respect to leeches, and yet these were by no means so often used as might be expected, nor in such large quantities as was once the rule. The medicines chiefly employed were mercury, opium, ipecacuanha, turpentine, the last both internally and externally, sprinkled over flannel or moist spongia-piline and applied to the abdomen.

The general outline given of this work is sufficient to indicate its value in illustrating several important questions in the practice of midwifery. Each subject is accompanied throughout with tables, which will render it of much value in a statistical point of view. The 'Report' is, indeed, a very important addition to those 'Reports' already furnished by the same hospital.

Lectures on the Diseases of Women. By CHARLES WEST, M.D. & F.R.C.P., Physician-Accoucheur to St. Bartholomew's Hospital, &c. Part II. *Diseases of the Ovaries, &c.* 8vo. (London: Churchill, 1858, pp. 257.)

This volume completes the course of which we noticed the first instalment in a previous number (XXIV, p. 301). The subjects treated of are inflammation of the uterine appendages and of the pelvic cellular tissue, uterine hæmatocele, inflammation of the ovaries, ovarian tumours and dropsy, and diseases of the bladder, urethra, vagina, and external organs of generation.

Dr. West pays considerable attention to the subject of ovarian tumours, and gives us a fair view of the present state of our knowledge respecting it. In the matter of treatment, however, we do not receive much help. Any attempt to dispel the tumour by preparation of iodide of potassium, &c., is utterly discountenanced. Ovariectomy is discarded, and the reasons are as follows:

"1st. The rate of mortality from the operation does not appear to be in course of diminution, as the result of the accumulated experience and increased dexterity gained by its frequent repetition.

"2d. Unlike most operations in which anything like the same rate of mortality occurs, it is scarcely admissible in the doubtful or desperate cases to which the Hippocratic axiom, "*ad summos morbos, summæ curationes*," applies. The cases in which it may be hoped that the disease, if left alone, will advance tardily or become stationary, those in which something may be anticipated from other less hazardous forms of interference, are the very cases that yield the successes on which it has been sought to establish the merits of ovariectomy. It is proved to be very hazardous indeed in the young; it is believed by some very competent surgeons to be attended by so much danger in those past the middle period of life, that they have proposed to regard the operation as contra-indicated in all women who have exceeded the age of forty-five years. The compound cysts, the cysts with solid matter, the malignant and quasi-malignant growths, those, in short, whose rate of progress is commonly most rapid, which are the most burdensome to the patient, are attended by the greatest suffering, and admit of the least palliation by other means, are precisely the cases in which the surgeon shrinks most from ovariectomy. In the table drawn up by Mr. Humphry, who himself is an advocate of the operation, cases of this description yielded when operated on 19 deaths to 20 recoveries; in my own table, deduced from a larger collection of facts, 56 deaths to 62 recoveries.

"3d. Not only is the operation so hazardous in those very cases where it is really most called for that many surgeons shrink then from its performance, but even in instances that may be selected as the most favorable we have no sure grounds on which to rest our prognosis as to its issue. "It is, in short, a venture at hap-hazard, since the medical practitioner is never able, in spite of the large experience already accumulated, to foretell the issue of the operation with the same certainty as guides him in undertaking other serious surgical proceedings. It has, indeed, been seen in numerous instances that extirpation of the ovary, though performed under the most favorable conditions, and by the most skilful hand, and without the occurrence of any untoward accident, has yet ended in a few days, sometimes even in a few hours, in the patient's death."

"These three reasons—the high mortality which experience and dexterity have failed to lessen, the special hazard attendant on those cases where yet the operation is specially indicated, and the utter uncertainty in which we find ourselves, even in the most favorable cases, as to its probable result—have chiefly influenced me in the formation of my opinion as to the general inexpediency of performing ovariectomy."

Tapping is tolerated, but only as a palliative measure. In most of the cases in which Dr. West injected a solution of tincture of iodine into the cyst the march of the disease was retarded, and in 2 cases out of 8 a cure was brought out. This plan, however, though not sufficiently successful, is only available in simple ovarian tumours, and, discarding ovariectomy, we are still left to proceed without any attempt to save life in the largest number of such tumours.

In contesting a statement, Dr. West frankly gives his opponents the

advantage of what he may have observed, however much it may militate against his own views. Thus, while taxing Dr. Bennet with exaggerating the frequency of ulceration of the neck of the womb, our author, nevertheless, gives the results of his own statistics, which show ulceration of the neck of the womb to be even more frequent than Dr. Bennet had supposed. In like manner, while affirming sub-acute inflammation of the ovaries to be a rare affection, he admits the frequency of inflammatory lesions of the ovaries; thus, "In 21 out of 66 instances, in which I examined the uterus and its appendages in the adult, the ovaries themselves, or parts immediately connected with them, presented changes more or less due to inflammatory action," and at pages 48 and 49 are related conditions of the vesicles, which are admitted to be "of no great rarity, and of an inflammatory nature," besides a case in which droplets of pus were found in an ovary, the stroma being healthy. After establishing the frequency of inflammatory lesions on or about the ovaries, we do not understand how Dr. West can fairly say that subacute inflammation of the ovaries is very rare. This department of pathology is confessedly obscure, and comparatively young practitioners can remember the time when all the pelvic diseases of women were vaguely spoken of as inflammation of the bowels or internal inflammation; and although inflammatory affections of the neck of the womb had attracted great attention, very little notice had been paid to the inflammatory affections of the ovaries before Dr. Tilt brought forward his views in a prominent manner—views which have been confirmed to a great extent by subsequent observation. Thus, Dr. Tilt laid claim to having forcibly exemplified "the great importance of *ovarian peritonitis* as a cause of disordered menstruation," and to have demonstrated not only its possibility, but the frequency of its occurrence. Now, this statement has been confirmed by Dr. Bernatz, who, in two papers, published in the 'Archives Générales de Médecine' for March and April, 1857, demonstrated by post-mortem examinations that many of the cases described by Thoriât and other French pathologists as instances of inflammation of that scanty amount of cellular tissue covering were, in reality, examples of *pelvic peritonitis*, and that the swelling detected around the womb by the finger depended on a certain amount of pus and false membranes lodged in that portion of the peritoneal cavity which lies adjacent to the womb. Dr. West affirms "that Dr. Bernatz has fallen into the error of stating as a rule what is indeed a somewhat rare exception," but in making this assertion our author cannot be aware that this distinguished physician of "La Pitié" has met with 97 instances of pelvi-peritonitis in the course of three years of hospital practice. Many of these cases occurred at the menstrual period, and could be attributed to no other cause than morbidly performed menstruation. Many occurred in the progress of gonorrhœa, and in these the recurrence of the menstrual nîsus often gave rise to an exacerbation of the pelvi-peritonitis. Not only does Dr. Bernatz's discovery fully support one of Dr. Tilt's main assertions, but it explains the frequency of peritoneal lesions in the vicinity of the ovary, as found by Dr. West, Dr. Renaud of Manchester, and many other observers, and also the well-known fact that such lesions are much more frequently met with in the female than in the male pelvis.

Another point mooted in Dr. Tilt's first edition was the frequency of the effusion of blood into the peritoneal cavity at the menstrual epochs. Since this was reported, another malady has been discovered, *hematocele*. This consists in a collection of blood deposited either in the cellular tissue between the folds of the broad ligaments, or in the peritoneal cavity itself. This complaint, which appears to be far from rare in France, was clearly investigated by Bernatz, Nelaton, Vignes, and others, and first described in England by Dr. Tilt in his second edition of '*Diseases of Women*.' Dr. West has met with four instances of it. With regard to the symptoms of this disease our author correctly observes :

"The acute symptoms scarcely ever appear till after the sanguineous discharge has either ceased completely, or has become much diminished in quantity. The symptoms are those of general febrile disturbance, seldom, however, very severe, accompanied by abdominal pain, and usually by enlargement of the abdomen. Even of their own accord, these febrile symptoms usually subside, and the pain also diminishes ; a sense of weight in the pelvis, bearing down, difficult micturition, and still more difficult defæcation remaining behind, and leading, by the distress which it occasions, to a vaginal examination, and to the discovery of the pelvic tumour."

Although this complaint has been known to occur independently of menstruation, still it is almost always connected with it, the menses being suppressed or retarded ; whereas, in some cases, the effusion of blood in the peritoneum coincides with menorrhagia. There is still some uncertainty respecting the manner in which menstruation produces this complaint. Besides the rupture of varicose veins in the broad ligaments, and hæmorrhage from the Fallopian tubes, enumerated by Dr. West, there is another condition considered most frequent by Dr. Tilt, that is, the softening and disruption of the ovarian stroma in an ovulating vesicle, an explanation which has also been submitted to the Académie des Sciences by M. Langier. This explanation rests on what has been observed after the patient's death in a certain number of cases, and on the assertions of Pouchet and other physiologists who have sometimes found the tissues surrounding a vesicle, which had discharged its ovule, so soft that it would break down on the slightest pressure. With regard to the treatment of this singular affection, some open the tumour with a trocar, but it is generally admitted to be best to leave it untouched, and favour the absorption of the blood by such measures as may best support the strength of the patient.

Although we differ from Dr. West on some points, we can strongly recommend his work as embodying the experience of a wise observer in an unusually wide field of observation.

The abolition of Craniotomy from obstetric practice. By Dr. TYLER SMITH, Obstetric Physician to St. Mary's Hospital. ('Medical Times and Gaz.', 12th Feb., 1859.)

In this paper the author shows that craniotomy is resorted to in British practice about once in every 340 labours. The whole number of births in England and Wales exceeds 600,000 per annum; and if we apply the proportion of 1 in 340 to these figures, we get a total of about 1800 cases of craniotomy per annum. This is as though every year all the children born in London during rather more than one week were sacrificed; or as though all the children produced during the year in such a county as Westmoreland were born dead. The mortality to the mother from this operation is nearly 1 in 5, in British practice, which would give in England and Wales a maternal mortality of between 300 and 400 per annum. Craniotomy is performed about twice as often in British as in French practice, and four times as often in this country as it is in Germany. It is an obvious fact that every improvement which has ever been made in obstetrics has tended to restrict and diminish the cases and conditions in which the performance of craniotomy has been resorted to. It is the author's object to show that, with the proper and scientific use of all the means at our command, it may be laid down as a general rule, that craniotomy should not be performed in the case of a living fœtus after the period of viability has been reached. It is certain that, up to the present time, the measures which are the alternatives of craniotomy have never been carried out in practice to their full and legitimate extent. Turning was the first great obstetric operation which checked the voluntary destruction of the fœtus during labour. The objections to turning which some obstetrists entertain depend on an almost superstitious fear of the uterus—a fear mainly owing to ignorance of the nature of the organ, and of the laws under which it acts. The dread of introducing the hand into the uterus has prevailed almost universally. But, apart from the danger of infection, the hand of the accoucheur, properly guided, can do no more harm in the uterus than any portion of the fœtus of equal bulk. Restrictions of the most absurd kind have been laid upon the operation, and it has come to be almost limited to arm presentations and cases of placenta prævia. On the Continent, turning is the recognised practice in cases of difficulty, where the head is above the brim, beyond the reach of the forceps, when the os uteri is in such a state as to admit the hand, and when no serious distortion of the pelvis exists. The operation of turning in cases of moderate pelvic deformity was practised by Denman, but it was dealt with rather as an exception than a rule of practice until the matter was taken up by Professor Simpson. No unprejudiced person can read Dr. Simpson's papers on this subject without coming to the conclusion that turning may be performed in cases of moderate pelvic distortion at the full term, with comparative safety to the mother, and with a reasonable chance of safety to the child. It is also shown to be applicable to cases of greater deformity, in combination with the induction of premature delivery. Nothing has ever occurred in the history of turning which has so strongly

tended to enlarge its usefulness as the introduction of anæsthesia into obstetric practice. Under chloroform we can turn with comparative ease in cases of excessive sensibility of the os uteri and vagina, in arm cases in which the waters have been long expelled, and the uterus has closed upon the fœtus with spasmodic force. It renders turning practicable in cases of convulsions or maniacal excitement, and in all instances it makes the uterus comparatively quiescent, and thus averts the dangers depending on contraction and resistance during the operation. Turning is performed nearly three times as often in France and Germany as it is in this country. After turning, the next great step in opposition to craniotomy was the discovery of the forceps. Before the time of Chamberlain, whenever turning was impracticable, there was no resource in cases of difficulty except in craniotomy. But it may fairly be questioned whether the whole powers of this instrument have ever been fairly brought out, especially in this, the country in which it was produced. If we examine our standard works, we find more pains taken to show when this instrument is not to be used than when it may be. The cases in which the forceps may be used are those of moderate disproportion or distortion, whether the arrest is at the brim, in the cavity, or at the outlet of the pelvis; cases of arrest from failure of the labour pains, without any morbid condition of the parturient canal; cases of convulsions in which the os uteri is dilated, and the head sufficiently low to be within reach of the instrument; cases of occipito-posterior presentation, not otherwise admitting of rectification, and face presentations; cases of accidental hæmorrhage; and cases of rupture of the uterus, in which no great recession of the head has taken place. It should also be used at a comparatively early period in many of the cases which, if not assisted, run on to impaction from swelling of the fœtal head and tumefaction of the soft parts of the mother. The outlet and middle straits of the pelvis are the limits within which the short forceps should be used; at the brim the long forceps is the proper instrument. The forceps is used more than twice as often in France and Germany as it is in this country. The last, and it may truly be said the greatest, opponent of craniotomy is the induction of premature labour. The largest single source of craniotomy is deformity of the pelvis. Now, it may be asserted, without the possibility of contradiction, that in this great mass of cases it would be right and practicable at once and for ever to abolish craniotomy in the case of the living and viable fœtus. In all cases of known deformity, an examination should be made in the early or middle months of pregnancy, and the proper treatment of such cases should be the induction of abortion or of premature delivery. In cases of excessive distortion, where it would be altogether impossible for a viable fœtus to pass, abortion should be induced before the time of quickening. It would be quite impossible for intercourse and impregnation to take place in any case in which it would not also be possible to induce abortion with safety to the mother. In the very considerable number of cases of moderate distortion in which the diminished capacity impedes delivery at the full term, but would allow of the passage of a child at the seventh or eighth month with a chance of living, the introduction of premature labour is the only justifiable practice. Besides the great operations of turning, the forceps, and the induction of pre-

mature labour, there are other means by which, in special cases, the necessity for craniotomy may be superseded. One of the most simple is the rectification of occipito-posterior presentations. When the occiput descends towards the sacrum in the third and fourth positions, instead of turning towards the right or left acetabula, great difficulty is produced, particularly in first labours, or when the head is large. Recorded cases of craniotomy show that the want of this rectification, which is generally possible with the hand, the lever, or the forceps, often leads to perforation. Cases of hydrocephalus in the fœtus are among the most difficult to deal with in an attempt to abolish craniotomy; but here we have the proposal of Dr. Simpson to tap the hydrocephalic head, and in this way reduce it so as to allow of delivery without the destruction of the fœtus. In actual occlusion or insuperable rigidity of the os uteri, incision is a safer and better practice than craniotomy. While it is the object of the present paper to advocate the abolition of craniotomy in the case of the living and viable fœtus, there is undoubtedly a class of cases in which perforation may be practised beneficially—namely, in labours where the child has died during the course of parturition. No woman should be allowed to remain in difficult labour after the death of the child has been satisfactorily ascertained. Considering, then, the various means at our disposal in the way of preventing the necessity for craniotomy, the author unhesitatingly expresses his conviction that, as a rule of practice, craniotomy in the case of the living and viable fœtus should be abolished; and he believes that if all the resources of obstetrics in the way of prevention, management, and alternative treatment were properly wielded, the necessity for the operation would not arise.

IV.

REPORT ON PHYSIOLOGY.

On the Physiology and Pathology of the Nervous System. By Dr. BROWN-SÉQUARD. ('Lancet,' December 25, 1859.)

THE principal points which Dr. Brown-Séquard endeavours to establish in connexion with the physiology and pathology of the central nervous system are these:

1st.—Excitations of the anterior roots of the spinal nerves may be a cause of pain, because these roots, being motor, produce a *cramp*. The pain due to this cramp is what has been erroneously called *recurring sensibility*. Cramps, and several other kinds of painful spasms (of the uterus during parturition, of the sphincter ani in certain cases, &c.), are painful on account of a galvanic irritation of sensitive nerves accompanying muscular contractions.

2d.—Our movements seem to be guided by the peculiar sensations we derive from the galvanic irritation of certain sensitive nerves of muscles, while they contract.

3d.—The power of transmitting sensitive impressions exists in many parts which are not able to give pain or any other sensation when they are excited by our usual means of irritation: so it is with the gray matter of the spinal cord and with many parts of nerves, which, however, are conductors of sensitive impressions.

4th.—Hyperæsthesia is a constant result of certain injuries upon, or alterations of, the posterior parts of the cerebro-spinal axis, from the tubercula quadrigemina down to the lower end of the spinal cord.

5th.—The transmission of sensitive impressions, in the spinal cord, takes place chiefly through the gray matter, and partly through the anterior columns; but, before reaching the gray matter, the impressions, in a certain measure, pass through the posterior columns.

6th.—The conductors of sensitive impressions from the trunk and limbs decussate in the spinal cord, and not in the encephalon, as was universally admitted.

7th.—Although the spinal cord is greatly altered or injured, sensi-

bility, more or less diminished, may persist everywhere, on account of a peculiar arrangement of the conductors of sensitive impressions.

8th.—The various kinds of sensitive impressions seem to be conducted by quite distinct nerve-fibres, in the nerves and in the nervous centres, and the place of passage of some of these conductors in the spinal cord seems not to be the same as that of the others, but none of them go up to the sensorium along the posterior columns.

9th.—In the upper part of the cervical region of the spinal cord, near the medulla oblongata, most of the conductors of the orders of the will to muscles are in the lateral columns, and in the gray matter between these and the anterior columns.

10th.—The voluntary motor conductors decussate at the lower part of the oblong medulla, and not all along the median line of the base of the encephalon.

11th.—The posterior columns of the spinal cord have a great share in reflex movements, and this is the principal cause of the peculiar kind of paralysis so often observed in cases of alteration of these columns.

12th.—The effects of excitation of the vaso-motor nerves consist essentially in a contraction of blood-vessels, which is followed by a diminution in the quantity of blood, in the temperature, and in the activity of nutrition. The effects of interruption of continuity of the vaso-motor nerves (*i. e.*, their paralysis) consist essentially in a paralytic dilation of blood-vessels, which is followed by a greater afflux of blood, an increase of temperature, and a greater activity of nutrition.

13th.—As a great many vaso-motor nerve-fibres go up to the brain and to the cerebellum along the spinal cord, the medulla oblongata and the pons Varolii, the diseases or injuries of the various parts of the cerebro-spinal axis, besides symptoms concerning sensibility and movement, present symptoms depending upon irritation or paralysis of vaso-motor nerves: contraction or relaxation of blood-vessels, diminution or augmentation in the quantity of blood, increase or diminution of temperature, alterations of nutrition, of secretions, &c.

14th.—Besides the influence of the nervous system upon nutrition, absorption, and secretion, through the vaso-motor nerves, there is another, which seems to consist in changes in the elements of the tissues—changes producing various modifications in the quantity of blood attracted, and in the interchange of materials between the blood and the tissues.

15th.—The absence of the influence of the nervous system on any part of the body is hardly a cause of other alterations of nutrition than atrophy, while the irritation of the nervous system is a most powerful direct or reflex cause of a great many morbid changes in nutrition, secretion, &c.

16th.—The sympathetic normal and morbid changes of nutrition, secretion, &c., are reflex phenomena, the study of which shows how many diseases are produced by a reflex action, and how a rational mode of treatment might be arrived at.

17th.—The loss of consciousness in simple vertigo or in complete attacks of epilepsy does not depend upon a disease of the brain, but upon a contraction of the blood-vessels of the cerebral lobes—contrac-

tion due to some irritation of the vaso-motor nerves of these vessels, either by some direct cause irritating them in the base of the encephalon or the spinal cord, or by a reflex influence.

18th.—Much more frequently than has been imagined, all the following affections may be produced by a peculiar kind of irritation starting from almost any centripetal part of the nervous system: epilepsy, the various forms of insanity, chorea, catalepsy, hysteria, tetanus, hydrophobia, &c.

19th.—The medulla oblongata is neither the only nor an essential nervous centre for the respiratory movements.

20th.—There are a great many nerve-fibres and nerve-cells in the medulla oblongata, the pons Varolii, and the other parts of the base of the encephalon, which are not employed in the transmission of sensitive impressions or of the orders of the will to muscles, and are endowed with the singular property of producing, after even a slight irritation, a *persistent* spasm in certain muscles, and especially in the neck. Rotatory convulsions very often depend chiefly upon the production of such spasms, and of changes in the blood-vessels of certain parts of the encephalon.

21st.—The irritation of the auditory nerve may cause rotatory or simple clonic convulsions.

22d.—The conductors of the orders of the will to muscles, of the sensitive impressions and of the nervous influences to blood-vessels, decussating at different places in the cerebro-spinal axis, various symptoms are to be observed, depending upon either the irritation or the paralysis of these three kinds of conductors, according to the part of a lateral half of the cerebro-spinal axis where an alteration exists.

On the causes of Death after the removal of the "Point Vital" of the medulla oblongata. By Dr. E. BROWN-SÉQUARD. ('Journ. de Physiologie,' No. 2, April, 1858.)

Anatomists are aware, that there is a small **V**-shaped portion of gray substance in the **V**-shaped space which results from the bifurcation of the medulla oblongata, and that the "point vital" corresponds to the apex of this **V**-shaped portion of gray substance. It was called point vital, nœud vital, or point premier moteur du mécanisme respiratoire, because instant death is the consequence of its removal. The theory was, that this point was the centre and stronghold of life—that in it all the life of the animal was accumulated. Dr. Brown-Séquard, however, shows very clearly that M. Flourens has erred in ascribing this importance and this function to this part of the nervous system. He shows that death is not always the immediate result of removing the "point vital." He shows, that when death happens suddenly after this operation, it is due, in part, to the sudden stoppage of the movements of the heart in consequence of irritation of the medulla oblongata. He shows that the action of the heart and lungs may be arrested or enfeebled, as much by the irritation of the neighbouring parts as by the irritation of the "point vital" itself. He shows that the removal of the "point vital" does not suddenly put a stop to the movements of the heart, after

the pneumogastric nerves have been divided. He shows that the respiration and circulation may go on effectually and regularly, for several days after the removal of the "point vital;" and that, consequently, this point has no claim to be regarded as the focus in which the pretended vital force has its origin. He shows, also, that voluntary motion and sensation is not destroyed by the removal of the "point vital."

On the function of the Posterior Fasciculi of the Spinal Marrow.

By Dr. SCHIFF. ('Brit. and For. Med. Rev.,' July, 1858.)

M. Schiff records some important points on which he is at variance with Brown-Séquard. 1. While the latter denies that the white posterior columns convey sensitive impressions to the brain, Schiff is led by his experiments to the inference that the white posterior columns, and these alone, conduct the impressions of touch; while the perception of common sensation—*i.e.*, pain through stronger mechanical, chemical, or thermal agencies, cannot be produced by the posterior columns alone. 2. The gray substance conducts the impressions of common sensation—pain caused by strong pressure, burning, vulneration, &c. The perception of mere touch is not conveyed by the gray substance. 3. After the section of one half of the spinal marrow, or of both posterior columns, simple touching is not felt any more, but stronger impressions on the respective parts of the body are felt as pain. True hyperæsthesia, as contended by Brown-Séquard, does therefore, according to these inferences, not exist.

On the Spinal Cord as a sensational and volitional centre. By Mr. G. H. LEWES. ('British Med. Journal,' 9th Oct., 1858.)

The paper, of which the following is an abstract, was read at the recent meeting of the British Association, by Professor Owen. Doubts, it appears, have long existed in the mind of Mr. Lewes as to the commonly received opinions regarding the functions of the spinal chord; and he has been led to perform a series of experiments, which, with their results, are embodied in the paper. Against the hypothesis of the brain being the exclusive seat of consciousness the author has for some years gathered increasing strength of conviction, preferring the hypothesis of the sensorium being co-extensive with the whole of the nervous centres. From the mass of evidence furnished by experiments, all bearing on the same point, the sensational function of the cord acquire in his mind the force almost of a demonstrated truth. A few cases may be selected. Place a child of two or three years old on his back, and tickle his right cheek with a feather, he will first move his head aside, and then on the tickling being continued he will raise his right hand, push away the feather, and rub the tickled spot. So long as his right hand remains free, he will never use the left hand when the right cheek is tickled, or *vice versâ*. But if his right hand be held, he will rub with the left. The voluntary character of these actions is indisputable in spite of their

uniformity; they are prompted by sensation, and determined by volition. The action of the sleeping child, under similar circumstances, is precisely similar. Mr. Lewes tickled the right nostril of a three-year-old boy. He at once raised his right hand to push him away, and then rubbed the place. When the left nostril was tickled he raised the left hand. When the left arm was gently but firmly held down, and the left nostril was tickled, he raised the right hand, and with it rubbed the left nostril, an action he never performed when the left hand was free. The actions of the sleeping and the waking child are so similar that both must be credited with sensation and volition (and if not both, then neither must be so credited); in like manner the actions of animals before and after decapitation exhibit no more difference, as respects sensibility, than the actions of the waking and the sleeping child; so that here again, unless both actions are credited with sensation and volition, neither of them can put in a claim. A water-newt was decapitated; its actions were precisely the same before and after decapitation. After allowing a quarter of an hour to elapse, Mr. Lewes touched the flank with acetic acid. The movements at first were very disorderly. It ran about in great uneasiness, just as it had done before its head was off. It curled itself up, and seemed about to die. Some time afterwards it was again touched with the acid; it again became disorderly, and was then pushed towards the side of the box; but it did not move until pushed slowly forwards so that its flank might come into contact with the wood; it continued crawling slowly and with intervals of rest, its body curved outwards so as to continue in contact with the wood, and its hind leg pressed close to the tail, and thus it rubbed away the acid. There are two points noticeable here: first, the readiness with which a sensation of contact suggested a means of relief; secondly, that this was the only newt which, in the author's experiments, ever hit upon this plan, and this one did so as well without its head as with it. The author divided the cord of a newt between the fifth and sixth cervical vertebræ. The convulsions which followed were almost as severe as those which follow decapitation; but in this case the fore legs were tetanic, and the hind legs pressed close to the body. After a few minutes it tried to rise, but failed. Bubbles of carbonic acid were constantly expired. After fifteen minutes it turned completely round and crawled five steps forward, dragging the hinder segment after it like a log, the hinder legs not moving at all. This was repeated several times. In fifteen minutes more, sensibility was detected in the hinder segment. Here was a case which would have been pronounced very simple. Division of the cord had seemingly destroyed all power of voluntary movement in the limbs below the section. The hind legs seemed paralysed. When the anterior segment was irritated, the animal crawled away, dragging the motionless posterior segment after it. When this posterior segment was irritated, the animal did not crawl, but simply withdrew the limb or tail. If the tail or hind leg were touched with acetic acid, the whole of the posterior segment (in which volition was said to be destroyed) began to move, and the legs set up the crawling action, attempting to push the whole body forward, which

could not be effected, because the anterior segment was perfectly motionless. The hind legs, which never moved when the anterior segment was irritated, moved now in obedience to the spinal volition; and the anterior segment, which before seemed so energetic in its voluntary movements, was now perfectly unmoved. Each centre ruled its own segment. If the motionlessness of the hind legs when the animal crawled is a proof that voluntary power is destroyed in those legs, the motionlessness of the fore legs when the hind legs moved is equally a proof that voluntary power is destroyed in the fore legs. The real truth seems to be that each segment had its own volitional centre, and that the one is never affected by the other. The author has divided the cord of a newt near the centre of the back. Four days after the operation, the animal had so far recovered that no spectator could distinguish between the voluntary power of its two segments. When the flame of a wax match was brought near the cerebral segment, the fore legs set to work, and the animal crawled away, dragging the hinder segment along. When the flame was brought near the spinal segment, the hind legs worked, and the body moved sideways, the anterior segment remained perfectly quiescent. All other stimuli produced similar results. The author argues that the explanation here proposed of two independent volitional centres is far more consistent with the phenomena than the explanation offered by the reflex theory. One simple fact proved that the spinal cord is a sensational centre, namely, the fact that whenever sensibility is destroyed all actions cease to be co-ordinated. Every one knows how greatly our muscular sensibility aids us in the performance of actions; but it has apparently been forgotten that if sensibility be destroyed in a limb, by section of the posterior roots, the power of movement will be retained so long as the anterior roots are intact; but the power of co-ordinated movement will be altogether destroyed. With diminishing sensibility we see diminishing power of co-ordination, the movements become less and less orderly; and with the destruction of sensibility the movements cease to have their co-ordinated harmony. Now in the cases cited it is clear that this power of co-ordinating movements—sometimes very complex movements—is nearly, if not quite, perfect in the decapitated animal; therefore, if co-ordination implies sensibility, the conclusion seems inevitable that the spinal cord is a centre of sensibility. The whole case is summed up thus:—1st. Positive evidence proves that in decapitated animals the actions are truly sensorial. 2d. Positive evidence, on the other hand, shows that in human beings with injured spines the actions are not sensorial, but reflex. 3d. But as the whole science of physiology presupposes that between vertebrate animals there is such a general concordance that whatever is demonstrable of the organs in one animal will be true of similar organs in another—and inasmuch as it is barely conceivable that the spinal cord of a frog, a pigeon, and a rabbit should have a sensorial function, while that of man has none, we must conclude that the seeming contradiction afforded by human pathology admits of reconciliation. Either animals are unconscious machines, or decapitated animals manifest sensibility and will.

On the Perceptive Power of the Spinal Cord. By GEORGE PATON, M.D., of Galt, Canada West. ('North American Medico-Chirurgical Review,' May, 1858.)

What, are the phenomena manifested by an animal after removal of the cerebrum? Have we evidence that it is capable of performing distinct perceptive movements?

Experiment 1.—Dr. Paton removed the cerebrum of a frog with great care, and observed the phenomena. Respiration continued. The frog no longer manifested spontaneous motion, but remained in a shallow vessel, amid a little water, with its hind legs drawn up, the posture that frogs assume when they rest; but on being irritated, it moved with great vigour, and gave every indication of recognising the stimulus. On being seized by the foot, it struggled much to be relieved, and on being freed, bounded from the grasp, and made several leaps before it became quiescent. On irritating the integuments of its left cervical region, it croaked; on repeating the irritation, it again croaked, and scratched the part with its left hind foot. On touching with the point of a needle the integuments over the right scapula, it raised up its right hind leg, and scratched the part with its foot. On continuing the irritation, it again raised up its right hind foot, and pushed away the instrument with force.

Experiment 2.—The cerebrum of a frog was removed with great care, and the animal allowed to remain quiescent till the effects of the operation had subsided. Respiration continued. The frog had lost the power of spontaneous motion, as it did not move from the place on which it was seated till irritated, but it appeared most sensible to the touch, and gave every indication of recognising a stimulus. When placed upon its back, it immediately turned upon its face, and made several leaps before it became quiescent. When the author irritated slightly the integuments of its thorax on the right side, it pushed away the instrument with its right hind foot. On a stronger irritation being employed, it withdrew its body in the opposite direction, and leaped to a distance. When he compressed slightly one of the toes of its right fore foot, it withdrew its foot, and placed it below its abdomen. When he touched with the point of a needle the integuments of its left dorsal region, it scratched the part with its left hind foot. And when he touched with the point of a needle its right cervical region, it raised up its right hind leg and pushed away the instrument with its foot. In short, it comported itself as regards the sense of touch like an animal that had sustained no mutilation, the only difference being that a slight irritation was required to be employed before the perceptive movements were manifested.

Experiment 3.—Dr. Paton removed the cerebrum of a frog, dividing the medulla oblongata a little anterior to the origin of the par vagum. After the operation a few slight respirations were observed, which gradually ceased. Dr. Paton touched with the point of a needle the integuments of the right cervical region, and it quickly raised up its right hind leg and scratched the part with its foot. He irritated the integuments of its left dorsal region, and it raised up its left hind leg and pushed away the instrument with its foot, and then leaped to a

distance, and made several successive leaps before it became quiescent. On irritating its right dorsal region, it withdrew its body in the opposite direction, and scratched the part with its right hind foot. In short, it gave every indication of recognising the stimulus, and endeavouring to avoid it.

The author has performed the same experiment on many other frogs with similar results. The animal, after ablation of the cerebrum, losing the power of spontaneous motion, but continuing most susceptible to every irritation, and performing distinct perceptive movements in response to a stimulus. And experiments on the alligator, where the phenomena can be witnessed on a much larger scale, are equally conclusive in establishing the same doctrine.

“A large alligator (four feet long), being decapitated, the headless trunk, as on many former occasions, performed numerous actions, indicative of sensation, intelligence, and volition. Resting perfectly quiet, deprived of all the special senses, it possessed only the general sense of touch, and responded in an accurate manner to all tactile impressions, even the simplest. No extreme agent, neither pricking nor fire, was required to elicit definite and defined movements. The slightest touch with the finger seemed to be perceived by the whole trunk, the tail, and limbs, as perceived by their movements. The animal seemed to be aware of the nature of the touching body, which, if producing little irritation, was borne without any violent efforts to escape from it. But fire, punctures, &c., called into agonised action the body, limbs, and tail. The body curved in a manner so as to recede from the offending agent, and the limbs were directed so as to remove it. From its actions, far more impressive than words, it was evident that it judged accurately, as to the degree, duration, and place of painful or painless impressions.”—*Dr. Dowler on Nervous Action.*

“We perceive from these experiments that an animal is capable of perception and the manifestation of volition, after being deprived of its cerebrum; and that it is not necessary that the impressions received by the nerves should reach the cerebrum that sensations may be produced.

“The animal leaps on being touched, or raises its foot and scratches the part of its integuments that is irritated; or, if an alligator, directs its limbs to remove the cause of irritation, and judges most accurately concerning the stimulus to which it is subjected. And to assert that these are not senso-volitional movements, is to give a new definition to the term. There is in these acts—1, an evidence that the animal has perceived the impressions made on the extremities of the sensory nerves; and 2, a proof that it exerts distinct control over the muscles of voluntary motion, in regulating and directing them to the attainment of a specific end. Because if the animal did not feel, and had no power of volition, why should one limb be raised up, in this manner, in preference to every other, to the part that we touch with the needle? Or why should the hind foot be moved forward to the identical spot in the dorsal region that is irritated? Movements perfectly similar to what we observe in other animals, when a particular part of their body is irritated.

"It is no doubt true that the animal has lost the power of spontaneous motion, as it does not move till irritated. And it does not retain the recollection of impressions received by the nerves, as it does not learn to avoid the obstacles to its progress—functions which it is admitted belong to the cerebrum. But, in the absence of these powers, the animal evinces such design, and adaptation of means to ends in resisting every irritation applied to its body, that we must conclude it is capable of distinct perceptive movements.

"From these experiments, we deduce the following doctrines :

"1. That the spinal cord enables an animal to feel, and manifest its perceptions by performing definite and combined movements in response to a stimulus.

"2. That the cerebrum is superimposed on the spinal cord to act on the possession of these powers, and as the seat of memory and the intellectual faculties.

"3. That the associated movements of deglutition, respiration, sneezing, coughing, contraction of the iris, &c., depend on the principle of reflex action possessed by the spinal cord.

"It will be observed, from the preceding experiments, that our researches have been confined to the functions of the entire spinal cord, and not of a separate section ; as our object has been principally to ascertain if the movements performed by an animal after removal of the cerebrum be illustrative or not of the theory of an excitatory system. And we consider that the phenomena we have described satisfactorily establish the doctrine that cold-blooded animals, after ablation of the cerebrum, are capable of sensation, and of performing distinct perceptive movements, on the application of a stimulus. And this is the proposition for which we contend."

From other experiments and considerations, Dr. Paton supposes the particular seat of this perceptive power to be in the medulla oblongata and upper portion of the spinal cord.

On a Nervous System which suspends the movements of the small intestines. By Dr. E. PFLÜGER. ('Journal de Physiologie,' April, 1858.)

According to Dr. E. Pflüger, the effect of galvanizing a certain portion of the spinal cord, or the grand sympathetic nerves, is to put a stop to the peristaltic movements of the small intestines. On galvanizing one of the grand sympathetics, peristaltic movement is arrested throughout the entire length of the small intestines, and thus the result is analogous to that stoppage of the action of the heart which takes place upon galvanizing the pneumogastric. As in the case of the heart, also, the arrestment of movement is rapidly brought about—rapidly, but after a perceptible interval—and the state of the muscle is one of relaxation. As in the case of the heart, also, the normal contractions begin again a short time after the current has ceased to pass, if this current has not been passed for too long a time. Dr. Pflüger has also ascertained that the peristaltic movements of the small intestines are not arrested by galvanizing the lesser sympathetic

nerves, and that the peristaltic movements of the large intestines are not affected by galvanizing either the large or small sympathetics.

These results have been called in question by Dr. Biffi, an able Italian physiologist, but they are confirmed by Dr. Brown-Séquard, who also shows that Dr. Biffi failed to obtain the same results as Dr. Pflüger because he did not take care to limit the action of the galvanic current to the portion of nerve experimented upon.

On the Organs of Vision, their anatomy and physiology. By THOMAS NUNNELEY, F.R.C.S., Lecturer on Surgery in the Leeds School of Medicine, &c. (Eight plates and 179 woodcuts. 8vo. London, Churchill, 1858, pp. 373.)

Mr. Nunneley's work presents merits of a very high order; it is for the most part carefully digested, well conceived, and admirably executed; it is also profusely illustrated throughout. An excellent account is given of the structure of the eye; and almost every statement is verified or not, as the case may be, by actual personal observation. The results of years of careful examination of the most delicate structures of the eye are given; many most interesting, and not a few valuable facts are added to our knowledge; and many good suggestions thrown out.

The elaborate series of measurements of the globe of the eye, the peculiarly interesting and elaborate accounts of the structure of the retina, and the description of the crystalline lens, may be instanced.

The statements made by Mr. Bowman upon the structure of the cornea are not endorsed by Mr. Nunneley. He is unable to distinguish any anterior elastic layer as a separate structure, nor has he been more successful in discovering the corneal tubes so minutely described by Mr. Bowman.

In the section upon the anatomical structure of the retina, after fully treating of its several layers, he gives the following description of the yellow spot of Sæmmering:

"In attempting to solve the problem, I know that I may expose myself to the charge of presumption; but at the risk of this I will mention what I believe to be the correct anatomy of this part of the retina. I give, however, the results of my investigation with great diffidence, and only as what I believe to be correct; but having seen some of the conditions described as the normal living structure actually occur while examining the part, I think I need not hesitate to declare that these at least are post-mortem changes, and not part of the true living texture.

"In cold weather, and in certain bodies, the central dark spot, with the surrounding yellow margin, may be seen as late as thirty-six hours after death, but frequently all trace of it is lost in twenty-four; within this period it may commonly be seen more or less distinctly. It is always more vivid in the young adult than in the old, and better seen on the inner surface of the retina than on the outer; at times the exact appearance represented by Sæmmering is shown, but more commonly the folds and central elongated depression figured by

Dalrymple are found. Not unfrequently there can be no doubt of an actual foramen, almost always if the eye has been placed in spirit, Goadby's Solution, or anything that corrugates the retina; but not unfrequently the folds can be pulled out, and the retina be seen continuous between them. Much depends upon the mode and force with which the retina is exposed, but more upon the changes which the retina spontaneously undergoes almost immediately after death, and which in its minute structure, as in a previous paper I have said, may be seen to occur in all animals within a few minutes after removal from the body.

"So soon as the retina begins to be opaque it is evident some alteration in its contents has occurred, besides which the evaporation which ordinarily takes place through the cornea alters the relation of the retina to the other tissues, as may imbibition from the vitreous humour or the fluid the eye is placed in. It is more than probable had Sæmmering not examined the eyes in which he first found the spot almost immediately the body had been taken out of the water, before evaporation had occurred, his discovery would not have been made.

"The following description is derived from the examination of the eyes of children born both before and at the full term, and either stillborn or such as having shortly died, I could procure and examine at once, and particularly of the eyes of two adults, which I have removed for cancerous disease of the appendages affecting the fore part of the eye, and to effectually extirpate which the eyeball itself had to be removed; preparation for the examination being made before the operations were commenced, so that the almost living eyes were examined, and where certainly the appearances were such as actually existed during life, the retina being transparent.

"On making a transverse section of the globe behind the lens there is seen a minute dense black spot projecting from the surface of the retina, surrounded by an inner, darker, yellow margin, and an outer, nearly circular one, which gradually shades off into that of the retina. There is neither aperture nor folds, but presently, as the vitreous humour escapes, and the retina loses its support, two folds, exactly as figured by Dalrymple, are seen to form. They may readily be pulled out, but immediately form again and always in the same place and direction, and of the same size, with the black puncture in the elongated depression between them, so that there must be some structural cause for their formation. That it does not depend, in the adult, as has been suggested, upon the connexion of two of the larger branches of the central artery to the retina at these points is certain, for no branches run in the lines of the folds, nor are these retinal vessels so large in man as in many animals where the folds do not occur. They appear rather to result from the fibres of the optic nerve dividing as they pass outwards, and leaving a small space where the puncture is free from the large filaments, so that when the retina loses the support of the vitreous humour and falls into folds, having evaporated through the cornea, these folds form the edges as it does very soon after the eye has been opened, and is always seen to do after the eye has become flaccid, by the aqueous humour where the duplication takes place.

"Thus I think it may be unhesitatingly be said that these plicæ are a post-mortem effect.

"That the appearance of a foramen is equally a post-mortem change, I have as little doubt. An opening is at first sought for in vain, but if the eye be placed in spirit, Goadby's solution, water, or left exposed for a time, an aperture more or less circular, and larger or smaller, will be found in the centre of the yellow spot. The best way of developing this is to remove the sclerotic and choroid coats, and place the eye in Goadby's solution diluted by one half, which beautifully preserves for some days the macula lutea. Gradually, according to its strength and consequent contracting effect upon the retina, there is a development of the foramen centrale, resulting, as I believe, like the folds, from the absence at this spot of the mass of nerve-fibres, but the presence of the rods, the granular and true retinal elements, which are more acted upon by the solution than the fibres, and being weaker, give way.

"When the folds are pulled out, or before they form the dark central spot is seen to project as a minute papilla from the inner surface of the retina, and on the posterior surface is a corresponding, but proportionately less, depression. This is an appearance which becomes more distinct after the retina has remained some hours in Goadby's solution, when an aperture being formed, if a section be viewed in profile it resembles on a microscopic scale the form of a volcano, the aperture being the crater. If the inner surface of the fresh retina be examined with a two- or one-inch object glass, there is seen an irregular circle of a canary yellow or pale lemon colour, with a dark irregularly circular spot, which breaks up into detached portions, looking like patches of the black matter of the choroid coat upon the inner surface of the retina, which here appears slightly elevated, as though a little thicker than the rest of it, and there is clearly a continuity of the nervous structure over the inner surface of its expanse. With the half-inch glass the same structure is seen, but the yellow tinge now appears to result from the deeper colour at the spot of the irregular granular cells, of which the whole inner part of the retina consists, while the dark central part is clearly made up of an aggregation of the minute colouring cells of the choroid coat. The appearance of a distinct spot is, however, much less characteristic under the half-inch lens than it is to the naked eye, for the yellow appearance melts into the surrounding retina, and the dark central foraminal appearance is shown to be a patch of the choroidal colouring globules, on the inner surface of the nervous tissues; hence it is difficult instantly to recognise the exact spot, either by reflected or transmitted light. With the quarter- and still better the one-eighth object-glass, when the part is gently spread out and very carefully and lightly pressed upon with the cover (for the whole retina is too thick without this), the dark spot is resolved into distinct minute choroidal colouring globules, which, as I have stated, when speaking of the choroid coat in thin layers, appear of a yellow or faint lemon colour. Thus I believe the yellow colour of the puncture results from an intermingling of these minute choroidal particles with the granular cells, rather than a positive yellow colour of these cells themselves, as

has been asserted, while the central spot is clearly due to a denser, more connected patch of choroidal globules, and which by a little pressure and teasing out may be resolved into the separate globules, lying upon (within) the granular and cerebral cells of the retina, which are as thick and numerous at this spot as in any other part of its expanse. I believe also that the rods are equally continuous on the outer surface, so that the appearance of the punctum centrale and surrounding limbus luteus depends upon the addition of the minute choroidal colouring globules to the normal retinal elements, diffused more or less in the yellow margin, collected into a mass in the dark central spot. I incline to regard this much debated spot as a vestigiform remains of the spot where a large blood-vessel has passed through the retina in the development of the eye, and has carried with it some of the choroidal colouring matter (I forgot to say that in course of the blood-vessels in the adult retina, the colouring particles are more numerous and more prolonged than in the intermediate places), and which in after-life ceases to exist, the retina being then exclusively supplied with vessels from the central artery of the optic nerve, the direction of the folds of Dalrymple deriving some explanation from this idea or it may be an undeveloped analogue of the marsupium in birds; but if so, why it should not be generally distributed it would be difficult to say."

Adaptation of the Human Eye to Distances. By Mr. CHARLES ARCHER, Surgeon in the Bengal Army. ('Proc. of the Royal Society,' 1858.)

The following is a summary of the author's views:

1. The eye is adapted to varying distances principally by an alteration in the fibrous arrangement of the lens itself. Moreover, that when the lens is removed after an operation for cataract, the power of adaptation is nearly lost, and can only be exerted within very confined distances.
2. The purpose of focalizing light at short distances is doubtless assisted, as suggested by Bowman, by the contractions of the ciliary muscle, in its antero-posterior direction, bringing forward the ciliary processes.
3. As the posterior hemisphere of the capsule is firmly united to the hyaloid membrane, this portion must always remain quiescent, and therefore the antero-posterior contractions of the ciliary muscle must be very limited as regards the lens.
4. The ciliary muscle, being placed around the eye, and its fibres being of a somewhat plexiform character, the contractions of the muscle will relax those yielding portions of the eye placed within its circumference.
5. The relaxations of the ciliary processes will deprive the capsule of its firm support. It will be pressed forward by the lens, which will meet with no further resistance to the expansion of its short axis.
6. The lens itself, as microscopically described by Bowman and

Kölliker, is admirably adapted to the varying changes which take place in the capsule.

7. The posterior capsule being firmly united to the hyaloid membrane, the alteration in the diameters of the cavity of the capsule must take place from the periphery of the lens to its centre, and from behind forwards, but not from before backwards, on account of the close union of the posterior capsule to the hyaloid membrane.

8. To allow such alteration to take place without endangering the acromatism of the lens, the alterations in the plane of its long diameter must be synchronous with the alterations in the plane of its short diameter. To allow of this, the margin of the lens is free in the canal of Petit; were it not the case, chromatic aberration would result.

9. The elasticity of the capsule of the lens and the ciliary muscle are antagonistic; that on the ciliary muscle becoming relaxed, the capsule of the lens is free to exert that elasticity.

10. By the pressure exerted by the anterior hemisphere of the capsule, by means of the polygonal cells of Virchow, on the anterior face of the lens, the organ is able to fulfil all the requirements for adapting it to receive focalized light from long distances.

11. The polygonal cells of Virchow are placed on the posterior surface of the anterior hemisphere of the capsule with the view before mentioned, and they are arranged with their long diameters in an antero-posterior direction, that pressure may not injure their transparency, which would be the case if placed laterally.

12. These cells are not found in other parts of the capsule.

13. The fibres of the lens are serrated for the purpose of uniting either to other, so as to allow them greater freedom of motion without altering their ultimate relations to each other.

14. The ciliary muscle is very highly endowed with nervous matter to supply all these varying requirements.

15. By the above postulates, all the modern discoveries in the microscopical anatomy of the eye receive a distinct expression of their individual functions, and by so doing, adapt the organ of vision to the acknowledged laws of light.

On the possible reappearance of Rigor Mortis after this state has been destroyed by stretching the muscles. By Dr. BROWN-SEQUARD. ('Journal de Physiologie,' April, 1858.)

These experiments were made upon rabbits and dogs. They show, in opposition to the statements of Nysten and Sommer, that fully formed rigor mortis may, within certain limits, return after having been completely destroyed. This is seen in the following experiment:

Experiment.—A strong, healthy dog was killed by putting a ligature around the trachea. Death happened at 8 a.m., rigor mortis commenced at 11 a.m., and was fully established at 12.30 p.m. At this time one of the hind limbs was alternately flexed and extended until it was rendered perfectly supple. In less than two minutes afterwards the rigidity reappeared, and in less than five minutes the limb was nearly as rigid as the other, which had not been

flexed and extended. At 1 p.m. the same limb was again moved until it was perfectly supple, and again the rigidity returned as before, and not less speedily or perfectly. At 3 p.m. the experiment was repeated, and the only difference in the result was that the rigidity was established more slowly and less perfectly. At 3.30 p.m. the rigidity again returned after the limb had been made supple, but in this case it was not until after an interval of ten minutes, and then only imperfectly. On the day following, twenty hours after the commencement of the rigidity, the hinder limb, which had not been experimented upon, was made supple, and in this case the rigidity did not return.

On the connexion between Cessation of the Circulation and Peristaltic Movements in the Uterus and Intestines. By Dr. O. SPEIGELBERG, of Göttingen. ('Henle und Pfeufer's Zeitschs.,' 3 Reihe, ii, pp. 1—44, 1857.)

Experimenting upon a considerable number of rabbits, cats, and guinea-pigs, Dr. O. Spiegelberg finds that the uterus and intestines remain at rest so long as the circulation is not interfered with; that peristaltic movements begin when the flow of blood is interrupted or arrested; and that these movements pass off when the circulation is restored; and his conclusion is, that this form of muscular contraction is caused by the want of blood in the part. He finds, also, that the uterus and intestines contract more vigorously in animals which have been bled to death; that a cornu uteri which has been separated from its vascular connexions contracts more decidedly than the fellow cornu which has not been so separated; and that the contraction is less marked after tying the vena cava or vena porta, than after tying the aorta. From this last fact, Dr. Spiegelberg rightly argues, as does Dr. Radcliffe from other considerations (v. 'Epilepsy and other Convulsive Affections,' 2d ed., London, 1858), that this form of muscular contraction must be ascribed, not to the presence of venous blood rendered stimulating by carbonic acid, as does Brown-Séguard, but to the positive want of blood, which is made really stimulating by the presence of oxygen.

Dr. O. Spiegelberg, moreover, would explain the occurrence of parturient contractions by local changes in the circulation of the uterus, and not by changes set up in the nervous system—an explanation which he will find to have been anticipated by Dr. Radcliffe in 1850 (v. op. cit.), as may be seen by the following quotation:

"And, certainly, the doctrine of stimulation is not wanted to explain the parturient contractions of the uterus. At the time of labour this organ returns from the state of progressive expansion in which it had been during the period of pregnancy; and as *one* cause of the previous state of expansion would seem to be found in the increasing vital activity of the fœtus, so now *one* cause of the return from this state would seem to be found in the failure of this activity—a failure brought about, first in the mother, and afterwards in the fœtus, in consequence of the growth of the fœtus having then passed the limit beyond which it cannot pass without trenching upon the

supplies necessary for the proper nourishment of the mother. It would seem, also, that this return of the uterus from the expanded state, or, in other words, this contraction of the uterine walls, must compress the vessels going to the placenta—that the vital activity of the fœtus must suffer a corresponding depression from this interference with the sufficiency of the placental respiration—and that this depression must again lead to contraction in the uterus—for if this organ contracted in the first instance in consequence of a depression of this kind, there is no reason why it should not do so again. And, further, it would seem that this second contraction must lead to a third, and the third to a fourth; and that thus, the uterus acting upon the fœtus, and the fœtus reacting upon the uterus, contraction must follow contraction, until the completion of birth. Nor does it follow from this hypothesis that the uterine contraction should be unintermitting, for it is quite possible (this among other reasons) that the blood which is displaced from the uterus during contraction may temporarily ‘stimulate’ the system of the mother to a degree which is inconsistent with an unintermitting continuance of contraction in any of the muscles belonging to the involuntary system. At any rate, it is quite impossible, upon any rational view of parturition, to refer the contraction of the uterus to any ‘stimulation’ on the part of the fœtus, without ignoring the whole of the previous history of pregnancy.”

Experiments on the phenomena of Respiration. By Dr. EDWARD SMITH, Assistant-Physician to the Hospital for Consumption at Brompton. (‘Proc. of the Royal Society,’ 20th Jan., 1859.)

In this paper the author describes the quantity of carbonic acid expired and of air inspired, with the rate of respiration and pulsation in reference to the whole day and night, the variations of the day with and without food, and the variations from day to day, and from season to season. The total quantity of carbonic acid expired in the twenty-four hours was determined in four gentlemen in eight experiments, some of which were continued for eighteen hours, with short intervals for meals only; and others were made at the beginning of each hour and half hour during that period. The quantity of carbonic acid exhaled in the six hours of the night is 1950 grains, and the total amount of carbon exhaled in the twenty-four hours at rest varied from 5·16 to 7·144 ounces in the different persons. The effect of walking at two and three miles per hour is found to be equal to $1\frac{1}{2}$ and $2\frac{3}{4}$ times that during rest; and by making a computation of the amount of exertion made by different classes of the community, the author finds that in the non-laborious class the carbon was increased from 7·144 ounces when at rest, to 8·68 ounces, and in the laborious class to 12·19 ounces daily. During profound sleep the amount of carbonic acid is lessened to the extent of half of that of the average of the day. The variations of the day with food are so great that the maximum is one half more than the minimum, and in one gentleman it was nearly double the minimum, the greatest occurring after each

meal, but particularly after breakfast and tea, and the least immediately before the meals. During a fast of twenty-seven hours the minimum quantity was maintained almost without change during the whole period of wakefulness, but there was a rise at the periods when the quantity usually rose with food. The quantity of carbon evolved in twenty-four hours without food is 5.923 ounces instead of 7.144 ounces with food—a quantity equal to that contained in 20 ounces of bread. The blood and the secretions become unusually alkaline. The variations from day to day were due to temperature and the state of the system. Sudden increase of temperature caused a sudden decrease in the respiratory changes, which continued until the temperature rose. This was an ever-acting cause of variation, but was the greatest after the cold of the winter. The state of the system caused by changes in the proportion of waste and supply, varies the quantity of carbonic acid evolved on the following morning. A good night's rest, a feeling of health, good supply of food, and not too much exertion, give an increase on the following morning (hence there was usually a high state of system on the Monday), and the reverse under the contrary conditions. As these conditions vary from day to day, the amount of carbonic acid evolved varies every day. The variations which are due to season are very remarkable and important, since it was shown that the respiratory changes vary from season to season in a definite and periodic manner, and so that the greatest changes occurred in the cold season, and the least in the hot season, and with definite periods at which this variation begins.

Dr. Smith also shows the amount of carbonic acid evolved with the exertion of the treadmill.

Experiments on the action of Food upon the Respiration. By Dr. EDWARD SMITH. ('Proc. of Royal Society,' 10th Feb. 1859.)

The method adopted by the author in these researches is—1st. To take the food under examination apart from the influence of other food, and, therefore, before any meal had been taken. 2d. To take about the quantity usually taken by mankind. 3d. To ascertain the amount of carbonic acid expired and of air inspired, with the rate of respiration and pulsation in a state of perfect rest, and in the sitting posture immediately before the food for examination was taken, and to compare all results obtained during the action of the food with this basis quantity. 4th. To make an inquiry in from three to ten minutes after taking the food, and to repeat it every twelve or fifteen minutes, until the maximum influence was obtained; the conditions as to posture and quietude remaining unbroken. It is found, from the hourly variations of these phenomena of respiration, that such an inquiry could not be made correctly between the meals, on account of the incessant variations then occurring from the meals (a source of error in the results obtained by Böker and other observers); nor in the evening, since, at that period, all the phenomena declined; and that only in the morning before breakfast, and before the usual breakfast hour, could trustworthy results be obtained. It is found, also,

that the influence of food is evident soon after its introduction into the system, and attains its maximum within about two hours. The persons experimented upon were chiefly the author himself and Mr. Moul; but Dr. Frankland, Mr. Hoffman, of Margate, and Mr. Reid, of Canterbury, had also submitted themselves to the inquiry. The substances investigated were very numerous, and the experiments exceeded two thousand. The following is a list of the foods described in this communication: 1. Starch series—arrowroot, arrowroot and butter, arrowroot and sugar, commercial starch, wheat starch, gluten, bread, oatmeal, rice, rice and butter, potatoes and gum. 2. Fats—butter, olive oil, cod-liver oil. 3. Sugars—cane sugar, grape and milk sugars, cane sugar with acids and alkalies. 4. The milk series (cow's)—new milk, skimmed milk, casein, casein and lactic acid, lactic acid, cream, sugar of milk and lactic acid. 5. Alcohols—spirits of wine, brandy, whisky, gin, rum, sherry and port wine, stout, ale. 6. The tea series—tea, green and black; hot and cold, in different quantities, and with acids and alkalies; coffee; coffee leaves; chicory and cocoa. 7. Other nitrogenous substances—gelatin, albumen, fibrin, almond emulsion. It is impossible to give an abstract of all the results obtained, although the author, in this communication, limits himself entirely to a statement of facts, leaving all theories and practical application of the facts for another occasion; but the following are a few of the principal results: 1. Pure starch and fats do not increase the quantity of carbonic acid evolved; but, on the contrary, the latter somewhat lessen it. 2. These substances are either not found alone in nature, or they are not used alone as food, but are associated with other substances, which tend to call them into action, and which constitute a class which the author has termed "respiratory excitants." 3. The cereals, viz., wheat-flour, oatmeal, and rice, containing besides starch, albuminous products, gluten and sugar, have a great and enduring power in increasing the production of carbonic acid—an increase equal to, or exceeding, two grains per minute, and continuing upwards of two hours. 4. Milk, in its natural combination and in each of its elements, excites the respiration to an extent, from new milk, of nearly two grains of carbonic acid per minute. Lactic acid had the least influence, and then cream; but cream had more influence than butter. No artificial combination of the elements of milk produced the effect of the natural milk. Milk produces its effects in different degrees in those who like and who do not like it. 5. Sugars are most rapid and powerful respiratory excitants, so that $1\frac{1}{2}$ oz. of cane sugar gave an increase of about two grains of carbonic acid per minute, in less than half an hour. The addition of acid usually increased the maximum, while that of alkalies increased the duration of its influence. Milk sugar had less influence than cane sugar, and grape sugar less than either; but the latter still produced an increase of more than one grain of carbonic acid per minute. Thus of the hydro-carbons sugar must be classed apart from starch and fat. 6. Tea and coffee are powerful respiratory excitants, producing an increase of from $1\frac{1}{2}$ to 3 grains of carbonic acid per minute, and an effect which endured upwards of one hour. Acid, added to tea, made it more stimulating; and alkalies rendered it more

soothing; but a fixed alkali destroyed its influence. Chickory and cocoa have a similar but less powerful action; while coffee-leaves caused a diminution in the exhalation of carbonic acid of 1 grain per minute. 7. Alcohols differ in effect both in the different members of the class, and in different specimens of the same kind. Alcohol always increased the evolution of carbonic acid to the extent of less than 1 grain per minute. Rum, also, commonly had the same result, and good malt liquors produced an increase sometimes exceeding 1 grain per minute, and enduring more than two hours. Sherry wine commonly gave a small and sustained increase. Brandy and gin, and particularly the latter, lessened the quantity of carbonic acid evolved, while whisky varied with the different specimens. The inhalation of the volatile elements of alcohol, and spirits and wine, caused a diminution in the quantity of carbonic acid, and an increase in the vapour exhaled by the lungs. These various members of this heterogeneous class differ greatly in the amount of alcohol which they contain, as well as in their other elements, as sugar, gluten, acids, salts, and volatile oils, and ethers, and, in the author's opinion, should not be classed together. 8. Gelatin, albumen, and fibrin also increase the production of carbonic acid to a maximum quantity less than 1 grain per minute. 9. Thus nearly all nitrogenous foods are "respiratory excitants." 10. Foods may be thus classed in reference to this latter quality: Non-excitants—starch, fat, some alcohols, coffee-leaves. Excitants—sugar, milk, cereals, potato, gluten, casein, gelatin, fibrin, albumen, tea, coffee, cocoa, chickory, alcohol, rum, ales, and some wines. As above stated, the author does not discuss the mode in which they produce these effects, but he adduces several facts which may aid in forming an opinion upon the subject. The author also states that wherever there was an increase in the quantity of carbonic acid evolved there was also an increase in the quantity of air inspired; but that these were not due to increased rate, but to increased depth of respiration. He also finds that the same food produced effects differing in degree in different persons, and in the same person at different periods, and that such was also the case with the action of acids and alkalies upon the human system.

On the influence of the Vagus upon Respiration. By Dr. WILLIAM GILCHRIST. ('Med.-Chir. Review,' Oct., 1858.)

Experimenting with a large and powerful induction-apparatus upon thirteen rabbits in succession, Dr. Gilchrist arrives at the following conclusions:

"1st. That expiration is *not* the normal and constant result of a strong irritation of the vagus, as stated by Eckhard, Budge, and others; but that if it occurs, it is dependent upon some complication in the experiment.

"2d. That expiration is not produced by *very strong* currents, as stated by Aubert and Tschischwitz. With the powerful induction-apparatus, which I was enabled to use through the kindness of Prof. Du Bois-Reymond, I employed currents varying from the most feeble

to such a strength as has perhaps never been used in this experiment, and on no occasion have I found expiration as a result. I do not, however, mean to deny that expiration does occur sometimes in this experiment, but I have convinced myself that its cause is not the intensity of the current, since with the strongest, as with much weaker currents, I have always seen the same result. Moreover, besides this positive evidence against this view, I may add a fact of a negative kind, viz., that on one occasion Kölliker found expiration produced in a dog by a very weak current.

“3d. *That the constant and normal result of irritation of the vagi is inspiration.* In thirteen experiments, in which the nerves were prepared with the utmost care, this effect was most constantly observed by myself and others. In some animals inspiration was produced by a current of weaker intensity than what was required to produce the same in others; but I cannot say I have obtained any data to lead me to suppose that a connexion exists between the intensity of the current and that of the contraction, since with the strongest current the contraction has not appeared to be different from that produced by a weaker. With one nerve only the results were essentially the same. One fact to be noted regarding the contraction is that frequently—indeed, generally—it remains for a considerable number of seconds after the irritation has been suspended, and then one sees the diaphragm relaxing quite gradually, and as it were by stages; and then follows very frequently a tumultuous kind of respiration, in which the expirations are particularly deep. In one instance I opened the trachea so as to get a view of the glottis. Its motions were distinctly seen, and on observing it whilst a current was applied to the nerves, it was observed to close after the irritation had occurred, and to remain so for several seconds. This observation I repeated (in the same experiment) several times. This closure of the glottis must no doubt have some influence on the duration of the inspiration, inasmuch as if the glottis remained spasmodically closed, the diaphragm must remain in a flattened and contracted-like condition, even although the state of tetanic contractions produced by the current had ceased.

“Although I feel from these trials quite convinced that the general and normal result of the irritation of the vagi is an inspiratory action, I would have wished to be able to give facts proving the cause to which the expiration found by other observers was due. This, however, I have not been able to do, and although it would be possible to frame hypotheses for the explanation of such an occurrence, I forbear doing so, having no substantial facts on which to found an opinion.”

On facts which seem to show that many kilogrammes of fibrine are formed and transformed every day in the human body. By Dr. BROWN-SEQUARD. (‘Journal de Physiologie,’ April, 1858.)

Judging by the quantity of the secretions, the transformation of organic matter in the animal economy must be very considerable. This is well shown in the admirable researches of Bidder and Schmidt. Thus,

for example, in six hours the quantity of gastric juice secreted by a dog is equal to a tenth part of the entire weight of the animal; and for every kilogramme of this weight the quantity of bile secreted is about twenty grammes. There are facts also which show that fibrine disappears from the blood which traverses the liver and kidney; and these are the facts which are taken as the basis of this memoir. The calculations into which Dr. Brown-Séguard enters are somewhat elaborate; and the conclusion to which they lead is, that every day from four to five kilogrammes of fibrine disappear from the blood which passes through the liver and kidney—a conclusion which shows that the same quantity of fibrine must have been formed somewhere (in the capillaries of many organs, in the limbs, in some other parts, and especially in the muscles) during the same time, for under ordinary circumstances the quantity of fibrine in arterial blood, and in the blood of the superficial veins, remains the same. A kilogramme is 2·204 lbs. avoirdupois.

On the Acid Principle of the Gastric Juice. By M. BLONDLOT.
(‘Journal de Physiologie,’ April, 1858.)

M. Blondlot’s analysis of gastric juice is—

Water	96·71
Biphosphate of lime	0·60 = calcium 0·12
Chloride of calcium	0·32 = calcium 0·11
Chloride of sodium	0·16
Hydrochlorate of ammonia	0·36
Loss	0·05

One of the considerations which arrests the attention of M. Blondlot in this analysis is, that the calcareous salts are in such proportion that the amount of base is nearly the same in each—a fact which seems to show that both salts have originated in the reaction of hydrochloric acid upon neutral phosphate of lime. Now, hydrochloric acid can no more exist in a free state in the blood, which is alkaline, than can biphosphate of lime; but it exists in considerable quantity in combination with soda. Indeed, the hydrated chloride of sodium is of all others the salt which is most abundantly met with in the animal economy. Hence, it is probable that the salt is decomposed in the coats of the stomach; that hydrochloric acid, arising out of the decomposition, finding itself in a nascent state in the presence of neutral phosphate of lime (the existence of which in the blood—probably in a state of suspension—is unquestionable), determines the formation of the two calcareous salts which are met with in the gastric juice; and that the soda which remains in the blood furnishes the alkali which is necessary for certain secretions, saliva, bile, &c.

In this way of looking at the matter, we have another proof that the acid principle of the gastric juice is almost exclusively the biphosphate of lime.

On an imperfectly known function of the Pancreas, namely, the Digestion of Azotized Food. By Dr. LUCIAN CORVISART. (8vo, Paris, 'Victor Masson,' pp. 123, 1857-1858.)

Very little is known about the manner in which animal or nitrogenized food is digested in the *bowel*; and science has not advanced one step since the discovery of Purkinje and Pappenheim in 1836 respecting the solvent power of pancreatic juice upon such food—a discovery which has attracted very little attention.

The physiological and experimental investigations on intestinal digestion which are contained in this essay are of considerable importance. They justify, as it seems, certain *physiological propositions* and certain *pathological deductions*, which propositions and deductions are as follows :

I. *Physiological Propositions.*

1. Nitrogenized food is digested both by the stomach and the pancreas.

2. The pancreas is, as it were, a supplementary organ, whose action, after copious meals, is added to that of the stomach.

3. Both digestions are of the same nature, as any article of food subjected to either is transformed into the self-same nutrimentative product (albuminose or peptone).

4. The pancreatic juice has peculiar reactions under the influence of heat or certain agents, which reactions the *gastric juice does not present*. As this difference in the juices is found when they are both charged with peptones after digestion, it has been erroneously supposed that the peptones also differed.

5. When an article of nitrogenous food, or a portion of it, has undergone a thorough gastric digestion, the pancreatic juice no longer acts upon it, and does not transform it into another peptone.

6. The pancreatic juice is intended to act upon that part of albuminoid substances which has left the stomach before being transformed into albuminose.

7. The amount of action of the pancreas may in certain cases, be equal to that of the stomach.

8. If the mere quantity of secreted fluid were alone taken into account, the stomach might be looked upon as the more powerful, for the gastric juice is ten times more abundant than the pancreatic juice, but the latter is, to make up the difference, ten times richer in ferment (pancreatine).

9. The gastric juice has the advantage of a prolonged contact and stirring with the food; but the pancreatic juice has, on the other hand, the faculty of acting upon azotized aliments equally well, either in an alkaline, neutral, or acid state; it also acts three times more quickly than the gastric juice.

10. Everything is so disposed in the duodenum, that the pancreatic juice acts as soon as it comes in contact with the food; and everything is so arranged in the stomach that a large part of the food is

transformed into peptone, the remaining part being, at the very least, so prepared, as rapidly to undergo the pancreatic digestion.

11. This preparation, which varies according to the quality and quantity either of the food or the gastric juice, &c., consists sometimes in a simple imbibition, sometimes in a dissevering or an extreme division, and sometimes in a solution. Pancreatic digestion, being very rapid, is usefully assisted by this preparation, the stomach acting respecting the pancreas in the same manner as the teeth do respecting gastric digestion.

12. It is, however, to be noticed that the pancreatic juice is able to accomplish, unassisted, the digestion of food which has not been subjected to that gastric preparation or division; in the same way as the gastric juice can digest food without extraneous help. Hence, pieces of albuminoid substances, being *directly* placed into the intestine in a raw state—that is to say, without any preparation—are perfectly and completely digested, the process being, however, somewhat slow. The pancreatic juice can, by its own unassisted energy, carry on the digestion of nitrogenous food, without requiring the adjunction either of the intestinal juice or the bile, to gain digestive properties. The digestion of azotized food, performed in glass jars over the water bath by means of the pancreatic juice or isolated pancreatine, goes on in the same manner as in the duodenum.

13. When the gastric and pancreatic juices are separated, and act in succession, each performs its function completely, and the quantity of albuminose produced may thus be doubled.

14. But it is a remarkable fact, that when these two digestive ferments meet in a state of purity, the two digestions are no longer freely carried on. The mixture, far from doubling the produce, may reduce it to nought, for pepsine and pancreatine destroy each other under these non-physiological circumstances.

15. Nature, in the normal state, prevents this conflict by three distinct means—1st, by the pylorus, which separates the two ferments; 2dly, by the very gastric digestion through which pepsine exhausts and abolishes itself in the formation of peptone; 3dly, by the bile, which destroys the activity of the gastric ferment, as has been shown by Pappenheim.

16. Bile does not precipitate the peptone produced by the influence of the stomach so as to destroy digestion and necessitate its being again begun. On the contrary, the bile itself is precipitated by the acid of the gastric juice or of the chyme.

17. The nature of the nitrogenous food has much to do with the quantity of peptone which the two successive digestions can produce for the requirements of the economy. I have thus found in my experiments, that whilst muscine and caseine yielded almost one ounce of perfect peptone, albumen, or gelatinous textures, though given in the same quantity, yielded hardly half an ounce.

18. At the outset, gastric or pancreatic digestion destroys the most characteristic properties of the various albuminoid substances. It liquefies insoluble ones, deprives albumen of its coagulability, and caseine of its property of coagulating by rennet. It also deprives gelatine of its property of turning into jelly, and muscine of being

precipitated by chloride of sodium, &c. In short, it transforms all substances into albuminose and peptone.

The different kinds of albuminose, although their individual reactions are much less marked than those of the albuminoid substances whence they are derived, have, nevertheless, distinct characters.

19. The nature of peptones varies as the nitrogenous substances from which they are derived. This variety satisfies the different (plastic?) requirements of the economy.

20. The peptones which are most alike and most difficult to distinguish from each other, are, the albumen-peptone, musculine-peptone, and, strange to say, gelatine-peptone; just as if the articles of food from which these peptones are derived were less different from each other than is generally supposed. Fibrine-peptone and caseine-peptone are more easily distinguished from each other, and from the substances above-named. From the slight differences existing between azotized articles of food, or peptones, there arises a kind of unstable equilibrium, favorable to the work of assimilation performed by the tissues of the body.

21. The generic character of peptones is, that they are always soluble in water, be the latter acid, neutral, or alkaline, which circumstance secures an easy circulation in the organism. Heat does not coagulate peptones, and hardly any of them is precipitated by acetate of lead. Besides, they resist insoluble metallic combinations a great deal better than nitrogenous articles of food.

22. Peptones form a genus, as well defined as the albuminoid genus. It is, however, evident, that by the progress of science, their nature will eventually be more exactly determined than can be done at the present period.

23. Some physiologists persist in the erroneous belief that the stomach merely swells or divides the food without dissolving it. How can they, however, withstand the testimony of the scales, which plainly show that, even where the weight of the food is considerable every albuminoid article of food subjected to the action of the stomach is not merely divided, but dissolved, passes through the filter, and is absorbed by the membranes?

24. Others have maintained that the gastric juice, acting on nitrogenous food, produces only gelatine. They, however, lose sight of the fact, that the characters which place gelatine in a peculiar albuminoid class, have never been discovered in the chyme after a digestion of fibrine, caseine, musculine, or albumine, even when the chyme was neutralized; and that, moreover, gelatine itself completely loses its specific characters, in consequence of undergoing digestion in the gastric juice.

25. Others finally, resting on the ancient hypothesis, that the albumen of the blood is nothing but the digested matters themselves, maintain that the peptones are reduced to albumen, by losing their acidity—viz., by being neutralized. Such an error can hardly exist, except albumen and fibrine be alone taken into account, excluding all other aliments; as an incomplete digestion of the albumen and fibrine may lead to confusion. Crude albumen, in fact, always partly escapes gastric digestion; ill-digested fibrine is transformed into albumen

only (caseiform); these two cases excepted, if experiments be made on the produce of concrete and washed albumen, of caseine, musciline, or gelatine, regularly digested by the stomach, no doubt can any longer be entertained. These gastric peptones never contain any albumen.

26. The peptones either received or produced by the pancreatic juice, do not, any more than the latter, form any new albumen, and, whether they be primarily or consecutively acid, alkaline, or neutral, do not increase by an appreciable weight the coagulable albumen which the pancreatic juice, pure and without peptone, *normally contains*.

27. During the three hours which follow a meal (when digestive solution, transformation, and absorption are not much advanced,) the blood of the vena portæ (compared to the venous blood generally) does not become charged with a noticeable quantity of nitrogenous matter through digestive absorption; whilst on the other hand, the elements of the blood, globules and fibrine, become changed into albumen (caseiform) by a commencement of digestion, either in the intestine or the water-bath, under the influence of the alkaline pancreatic juice.

28. Now, if it be considered that, during the first three hours of digestion—1st, the pancreatic juice poured into the duodenum remains therein in a pure and active state; 2dly, that this juice can pass into the vena portæ (for absorption by the mesenteric veins is not suspended;) 3dly, that this same juice can act in such an alkaline medium as the blood;—if, moreover, it be considered that during those very three hours, a large portion of the globules and fibrine of the blood of the vena portæ is, weights remaining equal, transformed in that vein into albumen (which is a commencement of transformation similar to that which they would have undergone in the intestine under the influence of this same pancreatic juice), we can hardly refuse our assent to the hypothesis of *a true intra-venous digestion*, which hypothesis the author confidently puts forward.

29. No actually differential character has ever been pointed out between the nitrogenous matters which go by the name of extractive, and the albuminose which is generated by gastric or pancreatic digestion. Now, it should be noticed that the lacteals, *the vena portæ*, and *the hepatic veins*, which are its continuation, or, in other words, the vessels which most directly receive the product of digestion,—are by far richer in extractive matter (albuminose) than the rest of the blood. It may, moreover, be noted that they are also richer in glucose.

30. The nutritive richness of the hepatic vessels (albuminose and glucose being contained in them) may be explained by the gastrointestinal absorption, to which is energetically added prolonged intra-venous digestion, although the liver has no share in the process.

II. *Pathological Deductions.*

A. We may take it as almost certain that there exists (as regards albuminoid ailments) a duodenal dyspepsia, caused by the vitiation, insufficiency, or absence of the pancreatic juice, the symptoms of which appear only from the second or third hour of digestion, with a deeper-

seated pain than is felt in gastric dyspepsia. (See Propositions 1, 2, 3, 6, 7.) The internal use of pancreatine is indicated* in cases of pancreatic duodenal dyspepsia.

B. Secondary duodenal dyspepsia may be the result of an almost total absence of that kind of division which food, under the least favorable circumstances, undergoes by means of the gastric juice before that food has been transformed into peptone. Pancreatic digestion is then slower, just as gastric digestion is slower when the teeth have not duly performed their functions. This secondary pancreatic dyspepsia may be cured by the treatment suited to the primary gastric dyspepsia.

C. Another secondary duodenal dyspepsia may arise, either from an excess of gastric juice, or from a patency of the pylorus; for in these two individual cases the gastric juice reaches the duodenum in unfortunately retaining all its active properties, which latter are prejudicial to the action of the pancreatic juice. (See Propositions 13, 14, 15, and 16.)

D. A third duodenal dyspepsia may arise from deficient biliary secretion, this deficiency being followed by the same unpleasant effects as are noticed in the two preceding cases, on account of the non-destruction of the activity of the gastric juice in the duodenum.

E. A peculiar kind of dyspepsia, which might be called of the portal vein, or hepatic, may arise from the vitiation of the intravenous digestion.

F. Certain symptoms of dyspepsia, gastralgia, enteralgia, or hepatalgia, may erroneously be attributed to the stomach, the intestine, or the liver; these symptoms may simply be the result of the absorption of a too abundant, too active, or too irritating pancreatic juice by the vena portæ.

G. Bile, when it reaches the stomach, destroys the activity of the gastric juice within that organ, whether it penetrates the cavity pathologically through the pylorus or by the mouth and cardia. The knowledge of this fact may lead to the employment of bile to counteract the morbid superabundance of the gastric juice.

H. The economy is supplied with a variable weight of peptone, though the weight of different kinds of nitrogenous articles of food and digestive force remain the same, the weight of the peptones varying according to the kind of nitrogenous food. It is a great error in hygienics to esteem the trophic, or nourishing power of a nitrogenous article of food, simply by the amount of nitrogen it contains. The trophic, or alimentary standard of food is not so easily fixed.

* Last year Dr. Corvisart made some clinical experiments on the therapeutic use of pure pancreatine. The difficulties he met with are recorded in the 'Gazette Hebdomadaire,' of Paris, May, 1857, pp. 321, 322. Dr. G. Harley, who read a paper on Digestion (just twelve months after the above date) at the meeting of the British Association for the Advancement of Science, seems never to have heard of Dr. Corvisart's article on the subject. Dr. Harley maintains, in opposition to the latter physician's statements, that in the administration of duodenal ferment, it is not necessary to imitate nature, which prevents pancreatine from passing into the stomach. For the causes of the difficulties met with by Dr. Corvisart, and the means to overcome them, see Propositions 13, 14, and 15, paragraphs C and D of the summary, and page 51 of the Essay.

I. When it is more urgent to allay pain and irritation about the digestive organs than to restore muscular energy, the food should consist of that kind of aliment which is most quickly and completely dissolved, whatever be the amount of peptone it yields.

J. But when it is more important rapidly to restore muscular force than to allay gastro-intestinal pain, we should, on the contrary, give food which, the digestive force being the same, yields the greatest weight of peptone, though that food be likely to dissolve and digest slowly. (See Proposition 17.)

K. He who digests with one organ only (stomach or pancreas) is thereby put on half allowance as regards peptone; and he who eats only albumen or gelatinous tissue (instead of caseine or masculine, which yield double as much peptone) is also put upon half allowance; and, with a normal and equal digestive force, is only half nourished. (See Proposition 17.)

In the two preceding cases, an over-activity either of the one organ (first case), or of both organs (second case), may occur, and extract from the food the full allowance of peptone. But we must not long trust this extreme functional exertion; for any persisting over-activity must sooner or later end in exhaustion.

L. We should not give for a long time one kind only of nitrogenous food, not only because one kind of azotized aliment is not capable of repairing the waste of the organism, but also because the same article of food given exclusively and continuously (for a week for instance) no longer excites gastric secretion, and no longer fully undergoes the digestive transformation.

M. Most of the peptones upon which Dr. Corvisart has made experiments, have the peculiarity of not being precipitated by neutral acetate of lead. Now, in all cases where the albuminoid matters of the urine happen to be of the albuminose kind, they remain in solution, in spite of the acetate of lead used to precipitate them. They therefore mask the sugar more effectually than all other ingredients of the urine when the potash and copper test is employed. The presence of sugar may thus be overlooked when it really exists in the urine.

Experiments on Digestion. By Dr. HARLEY. ('British Med. Journal,' 16th Oct., 1858.)

In this communication Dr. Harley states that, contrary to an opinion lately published by Bernard, he has found that the human saliva contains both sulpho-cyanide of potassium and iron. The latter substance, however, can only be detected after the organic matters contained in the secretion are destroyed by burning. Dr. Harley has ascertained that a person of nine stone weight secreted between one and two pounds of saliva in twenty-four hours. The gastric juice, the author says, does not destroy the power possessed by the saliva of transforming starch into sugar; consequently, the digestion of amylaceous food is continued in the stomach. The gastric juice has the property of changing cane into grape sugar. The author makes some remarks upon the cause of the gastric juice not digesting the living stomach; and says that his experiments show that it is

not the epithelium lining the organ which prevents its being digested, but the layer of thick mucus which covers its walls. When the latter substance is absent, the gastric juice attacks the walls of the living stomach, and digests them, causing perforation and death. As regards the bile, it seems that this secretion takes an active part in rendering the fatty matters of the food capable of being absorbed into the system. The most curious of all the digestive fluids, however, is the pancreatic secretion, for it unites in itself the properties of all the others. It not only transforms starch and other such substances into sugar, but it emulsionizes fats, and even digests protein compounds. As a remedy in indigestion, pancreatine should be greatly superior to pepsine, which can only digest one kind of food, namely, protein. The author says he has been labouring to obtain pancreatine in a perfectly pure state, and has been to a certain degree successful. With pancreatine, we should be able to digest any kind of food we pleased; and therefore the obtaining of it in a state of purity would prove an invaluable boon to suffering humanity.

Experiments on the transformation of Starch into Glucose in the Stomach. By Dr. F. G. SMITH and Dr. BROWN-SEQUARD. ('Journal de Physiologie,' Jan., 1858.)

In the 'Philadelphia Medical Examiner' for July and September, 1856, is a memoir (translated, also, in the present number of the 'Journal de Physiologie'), in which Dr. F. G. Smith relates experiments which seem to show that starch may be transformed in the human stomach into glucose without the help of saliva. These experiments were performed on the man (Alexis St. Martin) upon whom Dr. Beaumont experimented some years ago. The conclusion of Dr. Smith, however, has been called in question by Dr J. Dalton, of New York, who holds that the glucose proceeds from the bread used in these experiments. This led Dr. Brown-Séquard to inquire anew into the matter, and to call in the aid of the power he has of easily ejecting anything from his stomach when he wishes to do so. His experiment was this. At breakfast-time, after fasting fourteen hours, he washed out his stomach by drinking and ejecting three large glasses of water, and then he took a quantity of decoction made from carefully washed arrow-root. This decoction, tested carefully by Trommer's test, gave no evidence of glucose. Half an hour afterwards, a quantity of this decoction was ejected from the stomach, without any effort. This was semi-liquid, viscous, and very acid, and, with Trommer's test, it gave an abundant precipitate of oxide of copper. At different times, during the next hour, other portions of the arrow-root were ejected, and on each occasion the acidity and the quantity of sugar seemed to increase in amount.

A small quantity of the decoction of arrow-root which was not swallowed was left in contact with a certain proportion of saliva for half an hour, and in this case glucose was found, but in much less quantity than in the decoction which had been ejected from the stomach.

On the origin of Sugar in the animal economy. By M. SANSON, Professor of Chemistry in the Imperial Veterinary School at Toulouse. (Brown-Séquard's 'Journal de Physiologie,' April and July, 1858.)

In March, 1857, M. Claude Bernard read a paper before the Academy of Sciences, in Paris, the object of which was to direct attention to a special substance, to which he gave the name of *matière glycogène*, a substance which, under the influence of ferments, is easily transformed into sugar, and which is produced by the liver. In animals fed exclusively upon flesh, M. Claude Bernard maintains that this *matière glycogène* is produced exclusively in the liver; in herbivorous and omnivorous animals, he maintains that it is formed partly in the liver and partly at the expense of the starchy materials entering into the food. As to the rest, this *matière glycogène* is held to be a substance analogous to starch, and capable, like starch, of becoming transformed into sugar after passing through the intermediate condition of dextrine. At first it was supposed that this sugar-forming process was a vital process—a process inherent in the living liver; but subsequently M. Bernard has ascertained that it is carried on in the liver at a time when this organ may be regarded as dead, and hence it would appear as if, after all, the sugar-forming process were a chemical and not a vital phenomenon. At any rate, this is the inference which may easily be drawn, and which has been drawn by M. Sanson.

The arguments upon which these conclusions were based appeared to be very sound, and in a very short time they were accepted by physiologists on all hands. M. L. Fuguier, however, raised a dissentient voice, and showed that glycose was to be found in the blood of the general circulation, and that, on that account, glycose must be present in the flesh upon which the animals had been fed whose livers were supposed by M. Claude Bernard to have been solely concerned in producing sugar. This was, of course, an all-important point, for if it could be shown that glycose was supplied in the food, it could not be held that this substance was exclusively produced by the liver. M. Fuguier, however, did not succeed in making out his case to the satisfaction of the commission which was appointed by the Academy of Sciences to investigate it. Afterwards came MM. Colin and A. Chauveau, with additional evidence in favour of the existence of sugar, not only in the general circulation, but also in the majority of the animal juices, and especially in the lymph and chyle. Then, next in order, comes M. Sanson, whose first communication was addressed to the Academy of Sciences in May, 1857, and whose more matured views are contained in the paper which is now before us. The object of M. Sanson is to show that the *matière glycogène* of Professor Bernard is nothing more than dextrine, or a modification of dextrine which, like this substance, is capable of being transformed by diastase into sugar. This dextrine he meets with, not in the liver solely, but in the blood belonging to the general circulation, and in all the parenchymatous organs. In herbivorous animals it is produced by the

action of the saliva upon the starchy matters entering into the food; in carnivorous animals it occurs ready formed in the flesh-meat by which they are nourished. In a word, M. Sanson holds that the liver does not in any case create either sugar or *matière glycogène*.

The experiments and arguments of M. Sanson appear to show very satisfactorily the truth of these conclusions, but the commission appointed by the Academy—a commission consisting of MM. Bouley, Poggiali, and Longet—has given a report which shows that much remains to be done before we can consider that the theory of Professor Bernard is completely subverted. Thus, in a great number of experiments performed by the commissioners, and in some by M. Sanson himself before the commissioners, the presence of *glycogène* was detected only once in butchers' meat. In the flesh of horses *glycogène* was invariably present, as M. Sanson represents; but this fact, as the commissioners say, is not sufficient to show that this substance is always supplied in the food.

Under any circumstances, this paper of M. Sanson's is a most important contribution to the physiology of the sugar-forming process, and as such we earnestly commend it to the careful attention of physiologists and pathologists.

An experimental inquiry into the alleged Sugar-forming function of the Liver. By F. W. PAVY, M.D., Assistant-Physician to Guy's Hospital. ('Guy's Hospital Reports,' 3d series, vol. ii, 1858.)

The question to be discussed in this communication is not whether sugar is to be found in the animal system independently of a saccharine alimentation—for that the author considers to stand upon irrefutable ground—but whether the sugar encountered in the liver *after death* is a natural representation of the condition during life, or only the result of a post-mortem occurrence. As early as February, 1854, Dr. Pavy had noticed that the blood removed by catheterism of the right ventricle during life was almost completely destitute of saccharine impregnation. The observation did not then, however, receive the attention it deserved; but on repeating the experiment at a later period, and meeting with a similar result, an investigation was made, which has led to the conclusions advanced in this communication.

From upwards of sixty observations it is asserted, that the condition of the blood after death can no longer be taken as indicating its state during life; for if blood be withdrawn from the right ventricle of the living animal in a natural or tranquil state, there is scarcely an appreciable amount of sugar to be discovered; whilst, if the animal be afterwards sacrificed, and blood collected from a fine incision of the ventricle, it will be found to present a strong indication of the presence of sugar. In one of the experiments quoted there was a barely appreciable reaction in the blood removed during life, and nearly 1 per cent. of sugar in the blood collected after death, the animal having been sacrificed immediately after catheterism has been performed.

Observing this striking contrast in the blood abstracted from the right ventricle *before* and *after* death, the possibility occurred that there might be a corresponding contrast in the organ that was considered to be specially endowed with a sugar-forming function. The recent researches of Bernard had taught us, that a material naturally existed in the liver, which was extremely susceptible of conversion into sugar. It was this material, in fact, which was looked upon as giving rise to the sugar thought to be largely present in the liver during life. At the outset of the inquiry an agent was sought for which would check the transformation of the sugar-forming material after death, and thus present the liver in a condition as near as possible to that which existed during life. Potash was found to possess this effect, without destroying the principles concerned. A strong solution of it was then injected, as instantly after death as practicable, through the portal vein, into the liver; and, as the result, the organ presented scarcely any appreciable trace of the presence of sugar. A liver similarly treated when it had been allowed to remain a short period after death, gave the usual strong reaction of sugar that has been hitherto noticed. By injecting only a part of the organ with the alkali, it is most strikingly susceptible of demonstration, that the presence of sugar is in reality due to a post-mortem occurrence, and can therefore be no longer looked upon as a representation of the natural ante-mortem condition.

The sudden abstraction of heat from the liver instantly after death leads to a similar arrest of the production of sugar, and thus enables us likewise to represent the real condition of the organ belonging to life. In one of the experiments mentioned, where a dog was sacrificed, and a piece of the liver instantly sliced off, and thrown into a freezing mixture of ice and salt, the absence of sugar was almost complete; the amount, at least, was so small, that it was found impossible to arrive at a quantitative determination with a concentrated spirituous extract, notwithstanding the process is susceptible of so great a delicacy. The portion of the liver which was not submitted to the action of cold, and which was allowed to remain a short time in the animal, yielded, on analysis, an indication of 29.6 per cent. of sugar.

Division of the spinal cord in the lower part of the cervical region, the effects of which have been noticed by Bernard, but differently interpreted, leads to a corroboration of the deductions drawn from the preceding experiments. When the weather is cold or moderate, the operation is followed by a gradual reduction of temperature; and if the animal be sacrificed when its body has cooled down to about 70°, the liver is found free from sugar, upon an ordinary immediate examination, because at such a degree the *post-mortem* transformation is not effected with sufficient rapidity to lead to our deception; placed aside, however, it soon becomes strongly saccharine. Should the operation of division of the cord be performed, and the temperature of the animal be afterwards maintained at about the ordinary height by exposure to external warmth, then the liver is as strongly saccharine, upon ordinary examination after death, as if the animal had been taken and simply sacrificed.

By oiling the coats of rabbits, and exposing them to cold, the temperature of the body falls, and precisely the same phenomena are noticed as after division of the cord.

With frogs in a vigorous condition, the presence or absence of sugar in the liver submitted to the ordinary process of examination after death, is dependent upon the temperature of the animal at the time of the destruction of life. This fact was independently noticed by myself, about the time that it was mentioned by Bernard in a communication to the Parisian Academy of Sciences. Bernard's interpretation of it is connected with the relative activity of the abdominal circulation; but for myself, I bring it forward as strongly supporting the views that have been advanced, and consider it to be explained by the influence of temperature on the *post-mortem* production of sugar.

The material which occasions the presence of sugar in the dead liver has been called by Bernard "Glucogenic matter"—a term which, being only specially applicable after death, it is suggested should be abandoned, and replaced by Hepatine.

The amount of hepatine in the liver of the dog is much greater under a vegetable than an animal diet; the amount is also increased by mixing sugar with animal food. From the examples given it is shown likewise that the relative weight of the liver presents a proportionate variation, according to the quantity of hepatine present. In eleven dogs taken indiscriminately, that had been restricted to an animal diet, the weight of the liver was one thirtieth that of the animal. The average per centage of hepatine yielded by eight livers, also taken indiscriminately after an animal diet, was 6.97. Five instances have been collected of dogs restricted to a vegetable diet for some days prior to death. The average weight of the liver was one fifteenth that of the animal. In only three of the examples was the actual amount of hepatine determined, but in the other two it was noticed to be exceedingly large. The average given by the three was 17.23 per cent. Four dogs were placed upon an animal diet, and about a quarter of a pound of ordinary cane-sugar administered daily for a short period. The average weight given by the four livers was one sixteenth and a half that of the animal, and the average amount of hepatine yielded was 14.5 per cent.

The natural destination of hepatine in the living body remains to be determined. It has also to be shown how it resists transformation into sugar during life, when it is so rapidly changed at an elevated temperature immediately after death. A possible analogy may be presented by the following occurrence:—When a solution of hepatine, *in a neutral state*, is placed in contact with saliva, an almost instantaneous transformation into sugar takes place; but if a little acid alkali or carbonated alkali be added, scarcely a trace of change is for some time discoverable.

Under normal circumstances, rarely an appreciable amount of sugar is to be encountered in the circulatory system—only, according the author's analyses, from about .047 to .073 of a grain in 100 grains of defibrinated right-ventricular blood; and this would appear to result rather from a simple escape of a small amount of hepatine from the

tissue of the liver into the blood, whilst circulating through the capillaries, than from a special functional operation of the organ; for when a disturbance of the circulation, whether by congestion or the opposite, is occasioned, sugar makes its appearance to a considerable extent in the system, because the admixture of hepaticine with the blood is favoured. It can be easily shown by experiment, that on introducing hepaticine into the circulatory system, a saccharine state of the blood is induced; and if enough have been employed, a strongly marked diabetic condition of urine is established.

Sacrificing an animal, and maintaining the circulation by performing artificial respiration, occasions a well-marked diabetes. With the destruction of life, the transformation of hepaticine into sugar takes place, and this, being carried away by the blood, is eliminated by the kidneys, and thus renders the urine strongly saccharine.

Many phenomena which were before obscurely explained, receive a lucid interpretation from the new facts which have now been brought to light.

On the Functions of the Malpighian Corpuscles of the Kidney. By Dr. C. E. ISAACS, Demonstrator of Anatomy in the University of New York. ('Trans. of the New York Academy of Med.,' Vol. I., part 9, 1857.)

The general conclusion at which Dr. Isaacs arrives is, that the Malpighian corpuscles, or rather the glomeruli, are the principal secreting portions of the kidney. This conclusion is founded on numerous experiments, and on repeated microscopic and chemical examinations; and, so far as we can see, the evidence appears to be very satisfactory. At any rate, the memoir is one which must be thoroughly studied by all who would make themselves acquainted with the functions of the kidney.

The minor conclusions are: That several colouring matters are separated from the blood by the Malpighian bodies; that the colouring matter of the bile is thus separated; that lithic acid, in all probability, is thus separated; and that powerful diuretics give rise to a marked degree of congestion in the kidneys.

A Treatise on the Human Skeleton (including the joints). By GEORGE MURRAY HUMPHRY, M.B. Cantab., Surgeon to Addenbrooke's Hospital, &c. (Post 8vo, Cambridge, Macmillan & Co., 1859, with 260 illustrations drawn from nature.)

At Mr. Humphry's bidding, our old friends the bones forget their dryness, and are made subservient to many instructive lessons—lessons which are pleasantly told, as well as new and important. Of these the following are some of those which have interested us most, and which will give a good idea of the rest:

Mr. Humphry's description of the disposition of the fibres of the intervertebral substances and of the movements of the spine differs from that given by the Webers, and copied from them in most

anatomical works. He states that the forward and backward movement (flexion and extension) is most free between the 3d and 4th and between the 4th and 5th lumbar vertebræ. Here the lumbar curve is sharpest; the nervous cords are less closely connected with the vertebral canal than elsewhere; the spinous processes are large, and the interspaces between them are free and filled with stout, elastic ligaments, which have a powerful effect in bringing the bones back with a spring when the column has been bent forcibly forwards. Moreover, the projecting pubes and sternum in front, and the projecting sacrum and dorsal curve behind, enable both the flexor and the extensor muscles to act with great power upon this part of the spine. The movement becomes suddenly less above the 3d lumbar vertebra, and further diminishes towards the middle and upper part of the back, where it is very slight. It increases again in the neck; and the capability of motion backwards from the upright posture is, in the neck, greater than that of the motion forwards, whereas the reverse is the case in the loins.

Lateral inclinations, or flexion to either side, is permitted at every part of the column, but is most free in the neck and between those lumbar vertebræ where there is the greatest range of antero-posterior motion.

Rotary motion is slight in the neck; more free in the upper part of the back. It decreases towards the lower part of the back, and quite ceases between the 10th and 11th dorsal vertebræ—no rotation being practicable in the loins.

It is shown (p. 166) that the direction of these movements is in conformity with, indeed is dependent on, the disposition of the articulating processes in different parts of the spinal column; thus, the mode in which the articulating processes of one lumbar vertebra are embraced by those of the vertebra below, renders rotation between the two bones, upon a vertical axis, impossible.

Mr. Humphry devotes a chapter of his work to the "*proportions of the human figure*," and gives the following among other results of numerous measurements. The lower extremities are of great proportionate size, and, in both the upper and the lower extremities, the segments nearest to the trunk are comparatively lengthy—the more distal ones being comparatively short. The inferior animals, on the contrary, are remarkable for the comparatively greater length and strength of the more distal segments. This peculiarity in the proportions of the several parts of the limbs in man is attained during growth—the characters of the animal type being more and more relinquished as we approach adolescence. The inferior races of mankind exhibit indications of imperfect growth, not only in the deficiency of their stature, but also in the greater similarity of their limbs to the animal and fœtal type; thus, in the NEGRO, the forearms and hands, and the legs and feet, are longer in proportion to the arms and thighs than they are in the EUROPEAN. In rickety and deformed persons, also, the growth of the thighs and arms is usually defective, while the hands and feet attain nearly or quite their proper length. It is very remarkable, however, that in true DWARFS the growth of the thighs and arms rather exceed the normal relative

standard; whereas in GIANTS it is rather deficient. Hence, Mr. Humphry deduces the paradox that dwarfs, though undersized, are in reality *overgrown* persons; and the converse, that giants, though oversized, are rather *undergrown*.

We glean the following remarks on *fractures*: The bones are, in adults, weakest and most often broken at the narrowest parts, which are always at or near the points of confluence of the curves of their shafts. In elderly persons, however, expanded parts of the bones are rendered weaker by the disproportionate absorption of the cancellated structure, and fractures are consequently more common near the joints: and some instances which are given of fractures of the anatomical neck of the humerus in old persons form an illustration of this.

Spontaneous fractures (from muscular force) of the long bones are very rare, except in the case of the humerus; two instances, however, are given of its occurrence in the femur during the spasms that attended cholera, and four instances are mentioned of fracture of the sternum by the contraction of the muscles.

Although the clavicle lies close beneath the skin, the fractures of that bone are very rarely compound; this is accounted for by the fact that the outer end of the fracture, which receives the impulse, is almost always driven behind the inner one, into the loose cellular tissue of the neck.

Instances are mentioned of fracture of the glenoid cavity of the temporal bone by a blow upon the lower jaw, which drove its condyle into the skull, of the neck of the astragalus, and of the temporal bone lacerating the carotid artery.

Fractures of the spine are most frequent at the junction of the dorsal with the lumbar region; but at whatever point they occur, the upper portion is almost invariably driven in front of the lower.

Bony union, after fracture of the neck of the femur, fails to take place, not because the part is insufficiently supplied with blood, but because the bone is here surrounded only by a thin sheet of fibrous and synovial tissue, which affords no opportunity for the formation and lodgment of the materials from which any *external callus* might be formed; because the bony surfaces themselves are not retained in sufficiently close and steady apposition to permit of the slow process of *direct* osseous union, and because that process is still further disturbed by the admission of synovial fluid into the fracture.

Mr. Humphry adopts the plan of trephining the bottom of the acetabulum, so as to enable him to observe the condition of the *round ligament of the hip* in the various positions of the joint, and finds that the office of the ligament is not, as stated by Weber and others, to limit adduction of the thigh in the *erect* posture, inasmuch as when the thigh is extended the ligament cannot be rendered tense by any adduction or rotation of the thigh. In the *flexed* position of the thigh, however, the ligament is tightened by adduction or rotation of the limb outwards. He concludes, therefore, that the chief use of the ligament is to prevent too great inclination of the opposite side of the pelvis in alighting upon the limb in the bent position of the joint. He says that it is comparatively thick in foetal and early life, and

becomes thinner with advancing years, and that it is torn in each variety of dislocation of the hip. He observes that one of the uses of the notch at the lower part of the acetabulum is to afford room for the round ligament when the thigh is abducted or rotated inwards.

The *movements of the knee*, though apparently simple, are described by Mr. Humphry as being really very complicated and difficult to analyse, in consequence of two or more of them taking place at the same time. They are, 1st, flexion and extension; 2dly, rotation of the leg inwards and outwards—or pronation and supination of the leg—in the bent position; 3dly, turning of the tibia upon a *transverse axis* drawn through its upper end; 4thly, a sliding of the tibia backwards and forwards; 5thly, a rotation of the leg upon a *vertical axis*, which takes place during flexion and extension, and which is, therefore, to be distinguished from the movements of pronation and supination which takes place in the flexed position only. The 3d and 4th movements take place, as well as the 5th, during flexion and extension, and they are so combined as to keep the crucial ligaments tight in every position of the joint.

The offices of the several ligaments are stated to be as follows: All are rendered tense by extension; all tend, therefore, to limit that movement; and when the limb is straight they hold the joint firmly locked, preventing any movement but flexion. In the flexed state the lateral and posterior ligaments are all relaxed, and exert little or no influence upon the movements of the joint. The external lateral ligament does not limit pronation and supination of the leg, as it is often said to do. The crucial ligaments, being preserved in a state of tension at times, serve to hold the bones together when the lateral and posterior ligaments are relaxed. Moreover, the anterior crucial ligament prevents the tibia from being carried *forwards* upon the femur, either by the pull of the extensor muscles, or by any external force; it also limits *pronation* of the leg. The posterior crucial ligament prevents displacement of the tibia *backwards*. It is stronger than the anterior crucial ligament; and it needs to be so, because the flexor muscles, which tend to produce this displacement, have a more direct and powerful influence upon the tibia than have the extensor muscles, which tend to draw the bone forwards. This ligament also limits *supination* of the leg.

The semilunar cartilages are appendages to the tibia, and accompany it in its chief movements upon the femur; but in supination and pronation they adhere to the femur, and move with it upon the tibia.

The *short internal lateral ligament of the temporo-maxillary joint* is described by Mr. Humphry as a well-defined structure, though it has escaped the observation of anatomists. It arises by a broad base, a little external to and in front of the long internal lateral ligament, from near the same point of the spine of the sphenoid bone, and is attached, by a narrower apex, to the sharp ridge descending from the inner extremity of the condyle, immediately behind the insertion of the external pterygoid muscle. Like the *external lateral ligament*, it passes over the condyle, so as not to interfere with its forward or backward movement; and it combines with that ligament in limiting

the advance of the condyle forwards when the mouth is open, and in preventing its too great retrocession when the mouth is being closed. By fixing the neck of the bone, when the mouth is being opened, it also combines with the outer ligament in promoting the forward movement of the condyle. A line drawn through the neck of the jaw, between the points of insertion of these two ligaments, represents the axis upon which the jaw revolves during the opening and shutting of the mouth; and the condyle plays forwards and backwards between the two ligaments, both of which tend to prevent its displacement in a lateral direction as well as backwards or forwards.

On the Physiological Action of certain Poisons. By Professor KÖLLIKER.
(‘Virchow’s Archiv.’ t. 10, 1856.)

The poisons experimented with are curare, coniine, strychnine, opium, nicotine, veratrine, hydrocyanic acid; the subjects, frogs, with a few rabbits and dogs; the principal results as follows:

1. *The effects upon muscular irritability.* (a) Some poisons, as curare and probably coniine, paralyse the nerves in the interior of muscles without at all diminishing the muscular irritability. On the contrary, this irritability seems to continue longer than usual. (b) Other poisons, as veratrine, and probably extract of black hellebore, destroy the muscular irritability, but do not act upon the nerves. (c) Others, again, as hydrocyanic acid and its compounds, paralyse both nerves and muscles. (d) Muscles, the nerves of which have been paralysed by curare respond to a local stimulus only by partial contractions, which are somewhat of a tetanic character. (e) Muscles fatigued by tetanic contraction, which contraction may have been produced by opium, strychnine, or galvanism, are less irritable, and their irritability is sooner lost than other muscles which have not been so fatigued.

2. *The effects upon cadaveric rigidity.* (a) This rigidity is developed more slowly in muscles the nerves of which have been completely paralysed by curare. (b) This rigidity is developed more quickly in muscles the muscular irritability of which has been paralysed by veratrine. (c) Cadaveric rigidity is also developed more quickly in muscles which have been subject to tetanic contractions.

3. *The effects upon the heart proper, and upon the lymphatic hearts.* (a) Poisons which paralyse the nerves, as curare and coniine, have little action upon the heart proper. (b) The poisons which paralyse the muscular fibre paralyse the heart, and this organ becomes speedily rigid. When, moreover, the animal has been poisoned by hydrocyanic acid, the paralysis of the heart is accompanied by a degree of relaxation which is not met with when veratrine has been used as the poison. (c) The poisons which cause tetanus act very slightly upon the heart, except in cases where the tetanus has been caused by opium, and then each spasmodic paroxysm is accompanied by a pause in the action of the heart at the moment of diastole. (d) The lymphatic hearts of frogs are paralysed by the poisons which paralyse the peripheric nerves, and hence these hearts cannot be considered as

having an intrinsic principle of mobility. (e) During the tetanus which is caused by strychnia and opium, the lymphatic hearts are arrested in the moment of systole. (f) These hearts, also, are thrown into a state of tonic contraction, only more slowly than the rest of the muscular system, by the passage of a continuous current along the spinal cord.

4. *The effects upon the nervous system.* (a) The special action of certain poisons, as curare, upon the motor nerves, to the exclusion of the sentient nerves (at any rate these latter nerves, are not affected until a much later date, and then in a far inferior degree), points more clearly than any other experiment to the existence of two orders of nervous fibres. (b) Nerves completely paralysed by curare may as completely recover from this state. (c) Curare, coniine, nicotine, and prussic acid exercise their paralysing influence upon the motor nerves through the blood, the first three acting first of all upon the ramifications of the nerves, the last upon the trunks.

— In a word, the poisons have special affinities for certain organs—some for the nervous tissues and some for the muscular. There are, no doubt, other poisons which have special affinities for other organs, as for the blood, but as yet we know nothing of them. M. Kölliker divides the poisons which act upon the nervous tissue into three groups: (a) Those which act on the gray substance—veratrine, strychnine, opium. (b) Those which affect the tubular structure—curare, coniine. (c) Those which act on both elements—prussic acid, nicotine, ether. In each group some excite and others depress. It is doubtful whether any poison acts exclusively upon the muscular tissue, though veratrine may almost be entitled to this mode of action.

V.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

Further observations on the influence of Liquor Potassæ and other Alkalies upon the therapeutic properties of Henbane, Belladonna, and Stramonium. By Dr. GARROD, Professor of Materia Medica in University College. ('Proc. of the Med. and Chir. Society, 22d June, 1858.')

The object of this second communication is—

1st. To prove that the active principles of the plants under consideration are absolutely destroyed by the influence of the caustic alkalies.

2d. To show the ratio which must exist between the different preparations of the plants and the alkalies for the neutralization to be perfect.

3d. To ascertain the time demanded for the decomposition to be complete.

4th. To illustrate clinically the influence of the alkali in preventing the occurrence of symptoms, and removing such when large medicinal doses of these solanaceous drugs are administered.

Dr. Garrod, before proceeding to discuss these various heads, brings under notice a few points relating to the nature of liquor potassæ, and the properties of some of the officinal preparations of henbane, &c., showing that the former, although strongly caustic, still possessed but little neutralizing power, containing so small an amount of potash—not more than 6·7 per cent.; and that most of the preparations of henbane, belladonna, and stramonium, as the tinctures and extracts, were strongly acid in reaction, and hence, before the alkali could act upon the active principles contained in them, it must first neutralize this acidity, next separate the alkaloids from the acids with which they naturally are combined in the plants; that, therefore, much more was required (measured by the physiological or therapeutic strength of the drugs) to neutralize the galenical preparations than their alkaloids, or the active principles themselves. To prove that the active principles were absolutely destroyed by the alkali, he (Dr. Garrod) performed several experiments in the following manner: A solution of atropine was made by dissolving

it in water with the aid of a little spirit, dividing the solution into two parts, adding to one some carbonate of potash, to the other a sufficiency of liquor potassæ, and permitting both to remain for some hours. Chloroform was afterwards well shaken with both solutions, and allowed to subside, the supernatant fluid being poured off, and the chloroform washed with a little distilled water. Each portion was evaporated spontaneously in glass dishes. From the solution, to which carbonate of potash had been added, a gummy matter was obtained (soon, however, becoming crystalline), a solution of which dilated the pupil intensely; and when acidulated with hydrochloric acid, and chloride of gold dropped in, gave rise to the beautiful plumose crystals of the double chloride of gold and atropine. From the second solution, that to which liquor potassæ had been added, a strong-smelling substance was left, on the evaporation of the chloroform, having no power of dilating the pupil, and giving rise to no crystallization with the gold salt.

These experiments demonstrated beyond doubt the absolute destructive agency of the caustic alkali upon the active principles. It was also shown that most other alkaloids, as morphia, quinine, cinchonine, &c., were not so destroyed. To show the ratio which must exist between the different preparations of the plants and the fixed alkali, in order that neutralization may be perfect, Dr. Garrod gives the result of more than sixty experiments and observations in a tabular form, from which it appears that when atropine is acted upon by liquor potassæ, the destructive influence of the latter is so great that less than twenty minims are required to neutralize one grain of the former, and that probably pure potash will destroy its own weight of atropine. That when belladonna preparations are employed, the power of the potash becomes weakened, from the causes above alluded to, namely, the natural acidity of the drugs, and the necessity of first displacing the alkaloid from the acid with which it is combined; still, however, it was shown by the table that fifteen minims of liquor potassæ will destroy a fluid drachm of the tincture, and that twenty-five minims are sufficient to produce the same change in five grains of the extract; at once demonstrating that quantities very greatly beyond the medicinal doses of these drugs—indeed, even poisonous amounts—are rendered quite inert by very moderate addition of the alkaline solution.

The same is found to hold good in the case of daturine and the preparations of stramonium. Ten minims of liquor potassæ will neutralize a drachm of tincture of henbane, and thirty minims destroy nine grains of extract of henbane, although when ten grains are employed, dilatation will often ensue from a small portion of the extract, less than one grain being left free; and it should be observed that very minute proportions of these preparations are amply sufficient to induce the effect. Of course these extracts and tinctures are liable to variation in strength, acidity, &c., circumstances which must necessarily produce an alteration in the requisite amounts of liquor potassæ required for complete neutralization. With even the best extract, however, procured from one of the first druggists in town, it was found that nine grains were destroyed by the above-named quantity of potash. Nine grains of good extract of henbane and three fluid drachms of good tincture of the same, may be considered as doses of the drugs which few practitioners would prescribe;

yet these are destroyed by thirty minims of liquor potassæ, proving beyond all doubt that in the proportions prescribed in actual practice a total neutralization of effect ensues. To ascertain the required time, Dr. Garrod made experiments with solutions of atropine, and commenced the observations shortly after the addition of the potash. In an hour and a half the effect on the pupil was much diminished, and in two hours and a half ceased altogether. The influence of the alkali in preventing the occurrence of symptoms and removing the same when large medicinal doses of these solanaceous drugs are administered, was clinically illustrated by the narration of several cases, in which, after very decided effects had been induced by henbane or belladonna preparations, the addition of a very small quantity of liquor potassæ to the draught (the patient continuing the other drugs) quickly caused the cessation of the symptoms; and again, other instances where the withdrawal of the liquor potassæ from a combination was followed by the occurrence of powerful symptoms. From these observations and experiments Dr. Garrod concludes that the liquor potassæ possesses the peculiar power of destroying the active principles of henbane, belladonna, and stramonium, even when in very dilute solutions, and that the combinations frequently prescribed are utterly incompatible both in a chemical and therapeutical point of view.

Veratrum Viride as an Arterial Sedative. By Members of the Massachusetts Medical Society. ('Amer. Journal of Med. Sciences,' Oct. 1858.)

This paper is made up of mutual contributions from members of the Middlesex East District Medical Society, Massachusetts. From it we learn that the veratrum viride was introduced in 1835 as an important arterial sedative in most inflammatory affections, and that its reputation (which has been gradually extending since this time) is well deserved.

Veratrum viride (American white hellebore, Indian or meadow poke, itch weed) belongs to the order Melanthaceæ. It grows in moist localities, from Canada to Georgia; and, according to Dr. Gray, it is closely allied to the veratrum album, or true white hellebore of Europe. The root (which is the part used) has a sweetish bitter taste, and produces a persistent burning sensation in the mouth and throat and stomach. Applied to the skin it causes irritation, rubefaction, and even vesication. From the well-known drastic action of veratrum album, it was presumed that the v. viride had similar drastic properties; but general testimony goes to show, that it *very seldom, if ever, purges*. Considered chemically, it is found to contain gum, starch, sugar, bitter extractive, fixed oily matter, colouring matter, gallic acid, *an alkaloid identical with veratria*, lignin, and salts of lime and potassa. The alkaloid is nearly insoluble in water, more soluble in ether, most soluble in alcohol, and hence the tincture of the root is the preparation used.

On the therapeutic properties of Sarsaparilla. By Prof. BÖCKER, of Bonn.
(‘Edin. Med. Journal,’ Oct. 1858.)

Dr. A. M. Adam, in some interesting ‘Medical Notes from the Continent,’ refers to some interesting experiments by Prof. Böcker upon sarsaparilla, as yet unpublished. Dr. Böcker told Dr. Adam, “that after carefully performing ninety-eight experiments with this drug on healthy people, he found that, contrary to all our usually received opinions on the subject, it possesses neither diuretic nor diaphoretic properties. Another series of twenty-six experiments, on the persons of uncured syphilitic patients, gave exactly the same results. Böcker also satisfied himself that sarza does not increase the efficacy of the agents, such as iod. potass., &c., which are usually given along with it; and that the good results obtained by the administration of this salt, dissolved in decoction of sarza, are in no degree attributable to any virtue in the solvent fluid. I told Dr. Böcker that I remembered hearing Professor Syme, many years ago, express his opinion on the utter uselessness of so expensive a drug as sarza, remarking, in his own quaint, forcible style, that he believed an ‘infusion of hay’ would be just as good, and a vast deal cheaper. He seemed amused, and said that he entirely agreed with Syme; that infusion of sarza had no greater effect on the system than so much common tea; and that we must regard it merely as a pleasant, but very expensive, vehicle for the administration of other medicines.”

On the influence of mercurial preparations upon the Secretion of Bile.
By Dr. GEORGE SCOTT, formerly Physician to the British Hospital at Renkioi. (Beale’s ‘Archives of Medicine,’ No. 3, 1858.)

Kölliker tried the effects of calomel upon the secretion of bile in dogs with biliary fistulæ, but it is difficult to form any definite conclusions from his observations in this respect. *Once* the bile seemed to be *increased*, and *twice* it seemed to be *diminished* by the administration of calomel. Dr. Scott enters upon the same inquiry; and the result of four experiments, conducted with great ability and extreme care, is, that there is a *diminution in the amount of fluid bile and bile solids secreted after the administration of large doses of calomel.*

“In the first two experiments,” he says, “particularly in the second, the decrease was no doubt due somewhat to the diminished quantity of food taken the day after the calomel was given; but that it was not all dependent upon this cause seems to be pretty clear—1stly, from the fact of the bile of 19th June, the day following that in which food and milk were purposely withheld from the dog, having been considerably greater than the quantity secreted on the day following the 16th June, when also no food was taken, and when in addition 6 grains of calomel had been administered to the dog; and 2dly, from the bile in the last experiment, when almost exactly the same amount of nourishment had been consumed *after* as *before* the exhibition of calomel, having been also very much diminished in quantity. In three of the above experi-

ments, the bile-acids were also considerably *diminished* after the calomel; but in one, viz., the third experiment, they were *increased*. Why the bile-acids should be increased in the latter case, and diminished in the other three, it must be confessed it is difficult to explain.

"Although it would be rash to venture any decided opinion from the very small number of experiments above detailed, yet the few that were made all point so much to one conclusion that, if they be confirmed by future and more varied trials, they would throw considerable doubt upon the generally received opinion that calomel in large and purgative doses increases the flow of bile. It may be urged that although calomel does not increase the secretion of bile in the dog, that is no reason why it may not do so in man; and that, even if mercury do not excite the liver to increased secretion in a healthy state of the organ, it may still do so in certain diseased conditions of the same. If the first objection were true, the same could be brought against the results of the experiments which have been made upon the lower animals to ascertain the action of poisons or any other articles of the *Materia Medica*. With regard to the second objection, nothing analogous occurs in the action of drugs upon other organs; there is no medicine which diminishes the secretion of urine in the healthy state of the kidney, and increases the same in certain diseased conditions of the organ; there is no medicine which diminishes the amount of sweat in a healthy state of the skin, and acts as a diaphoretic in certain diseased conditions of the integument. Hence it seems difficult to suppose that anything which diminishes the flow of bile in a healthy condition of the liver, should increase the secretion of the same in a diseased state of that organ.

"Whether it be the mere purgative effect of calomel which causes the diminution in the secretion of bile, or some specific action, further experiments must decide. Of course, it must be understood that the above remarks apply only to cases where *purgative* doses of calomel have been given. Whether small and frequent doses of calomel continued for a length of time, so as to produce the specific action of mercury upon the system, really augment the biliary secretion, is matter for further experiment.

"The above analyses were made in the laboratory of Dr. Lionel Beale, to whose great kindness I am indebted for the opportunity of performing these experiments."

On the preparation of digestive powder from the pig's stomach. By Dr. BEALE, Physician to King's College Hospital. (Beale's 'Archives of Medicine,' No. 3, 1859.)

"Various chemical processes," says Dr. Beale, "more or less complicated have been employed in the preparation of pepsin. Partly in consequence of these being tedious and difficult of performance, and the results uncertain, and partly from the sale of perfectly useless preparations, the remedy has of late to some extent lost its reputation. Having been engaged in some experiments upon artificial digestion, and having met with considerable difficulty in obtaining clear solutions of digestive fluid that would filter, I tried various new plans of preparing digestive solutions. The following

answers very satisfactorily, is very simple, and free from many of the objections to which other processes are liable.

"The mucous membrane of a *perfectly fresh* pig's stomach is carefully dissected from the muscular coat, and placed on a flat board. It is then cleansed with a sponge and a little water, and much of the mucus, remains of food, &c., carefully removed. With the back of a knife, or with an ivory paper-knife, the surface is scraped very hard, in order to press the glands and squeeze out their contents. The viscid mucus thus obtained contains the pure gastric juice, with much epithelium from the glands and surface of the mucous membrane. It is spread out upon a piece of glass, so as to form a very thin layer, which is dried at a temperature of 100° over hot water, or in vacuo over sulphuric acid. When dry it is scraped from the glass, powdered, and kept in a stoppered bottle. A good digestive fluid may be made as follows :

Of the powder	5 grains.
Strong hydrochloric acid	18 drops.
Water	6 ounces.

"The fluid may be filtered easily, and forms a perfectly clear solution, very convenient for experiment. If it is to be taken as a medicine the powder may be mixed with an equal quantity of starch, and 10 grains of the mixture taken for a dose, a little diluted hydrochloric acid in water being taken at the same time. It may be taken with the salt at a meal. The powder is devoid of smell, and has only a slightly salt taste. This powder undergoes no change if kept perfectly dry. It contains the active principle of the gastric juice almost unaltered."

The therapeutic relations of Opium and Belladonna. By Mr. BENJAMIN BELL. ('Edinburgh Medical Journal,' July, 1858.)

The two following cases are calculated to set forth the advantages of the subcutaneous method of administering narcotic remedies, as well as to afford further proof of the antidotal relations of opium and belladonna—a point to which attention was called, first of all by Dr. Thomas Anderson—('Abstract' XXII, p. 303.)

"On the 10th of March," says Mr. B. Bell, "I was requested to visit one of the men belonging to the same institution, who had been afflicted, for nearly two months, with sciatica of the right thigh and leg. His sufferings were very severe, almost without intermission, and aggravated by motion of the limb. I attempted to treat him, in the first instance, constitutionally, with croton-oil pills, quinine, and iodide of potassium; but as no material improvement took place, recourse was had to the local treatment by injection. Twenty minims of a solution of morphia, double the ordinary strength, were introduced over the sciatic nerve, where it emerges from below the pyriformis muscle. He experienced immediate and complete relief, which continued for eight or ten hours; but at the end of that time the pain came back again as intense as before. The injection was repeated on the two following days, with benefit to this extent, that when lying quietly in bed he now had intervals of comparative freedom from acute suffering. The internal remedies were continued.

"The disease however, was very obstinate, and I determined to try the injection of atropia, which had answered so well in the case of the young woman already mentioned. With her, one twelfth of a grain had been employed without causing unpleasant symptoms, although the seat of pain was in the forehead; and I therefore inferred that in the present instance, where the symptoms were so much more obstinate, and at so great a distance from the brain, I might safely use three times the quantity. I accordingly injected one fourth of a grain of sulphate of atropia over the sciatic nerve. He experienced instantaneous relief, just as when the morphia had been used. This was at twelve o'clock. He said that he felt rather sick. I waited for some minutes, after which, as he made no further complaint, I left him in bed looking for a comfortable sleep, now that the pain was entirely removed. Upon reaching home, at half-past four, I found that a message had come sometime before, intimating that the blind man, on whom I had operated, was very ill, much excited, and unable to speak. I went immediately, and found him very much as described; his countenance and head extremely flushed, with great distension of the veins; the breathing hurried; the pulse rapid and small; the skin hot and bathed in perspiration; he was exceedingly restless, his hands incessantly moving as if engaged in some of his ordinary handicraft duties at the asylum; his hearing was evidently acute, as he attempted to reply when spoken to; but there was a remarkable dryness of the mouth and throat, which prevented articulation, even if the mental disturbance had been less complete. From the previously disorganized condition of both corneæ, the condition of his pupils could not be examined. It is perhaps worthy of notice, that the general surface of his body appeared to be very itchy, from his frequent endeavours to scratch it.

"His condition was altogether alarming, and there were no indications of any tendency to improvement. Under these circumstances, being acquainted with no more promising plan of treatment, I had recourse, with some confidence, to the subcutaneous injection of morphia. I injected without delay twenty-five minims of the double strength solution into the gluteal region of the opposite limb, which happened to lie next the edge of the bed. This was about five p.m. Almost immediately a decided change for the better was perceptible. He became considerably calmer, and swallowed a little water without much difficulty. I visited him again at half-past seven, and was glad to find that he had been sleeping quietly in one posture for an hour and a half. The remarkable flushing and congestion of the head and face had entirely disappeared. The pulse was fuller and less frequent; the skin soft and comfortable. He continued to sleep composedly until four next morning, when he awoke still rather confused; he again fell asleep, and awoke at six, apparently quite well and free from mental disturbance. When I called in the course of the forenoon, I found him entirely relieved from pain and in good spirits, but quite unconscious, or at least oblivious, of all that had happened during the period of so much anxiety to those around him. He had been out of bed walking up and down his room, quite delighted to find that he could now move about without any uneasiness in the affected limb, a feat which had not been possible for many weeks.

"A few days after these occurrences, a former patient, in whose case I had

used the atropia a month before, had a violent return of the tic in her brow, and was anxious to have the same treatment employed. I questioned her particularly as to her previous experience of the two remedies, and she gave a decided preference to the atropia. I used only five minims of a strong solution (gr. viii. ad ℥i.), or one twelfth of a grain,—the same quantity as on the previous occasion. She was suffering intense pain at the time, and as usual, the relief was almost immediate; but in a few minutes she moaned a good deal, and on being asked the reason, now that the pain was gone, replied, that she felt afraid, and saw a number of strange-looking people at the foot of her bed. Her pulse soon became frequent and rather small, and she had a considerable amount of subsultus and jerking of the hands. The quantity of atropia that had been injected was so small, that I had no hesitation in leaving her for a time; but I directed one of the other inmates of the asylum to encourage her by sitting at her bedside, in case she continued sleepless and uncomfortable. This was at mid-day. I saw her again at 2.30. She was much in the same condition—restless and moaning, with a frequent pulse, and complaining, when questioned, of an unpleasant feeling of deadness or want of power in the lower extremities, and of dryness in the throat. With her own ready consent, I injected fifteen minims of the strong solution of morphia over the right shoulder-blade. She soon felt much more comfortable, and after the lapse of two hours, the pulse, from being small and rapid, had become full and soft, although still more frequent than natural. On the following day she was comparatively well, and quite free from pain. She mentioned very distinctly, that, immediately after the morphia was injected over the shoulder-blade she had experienced complete relief from an unpleasant sensation in her head, and also from the feeling of deadness in her lower extremities already referred to.”

Experiments on the action of Caffein. By Drs. STUHLMANN and FALEK, of Marburg. ('Archiv. f. Path. Anat. & Physiologie;' and 'Amer. Med. Monthly,' Feb., 1859).

These gentlemen have made a series of thirty-eight experiments with caffein, on dogs, cats, rabbits, birds, frogs, snakes, and fishes, clearly showing that caffein is a poison that will kill in comparatively small doses, and in a short time. Thus five centigrammes (about $\frac{8}{10}$ gr.), introduced beneath the skin of frogs and toads, determined local irritation, sometimes slight excitation of the circulation, respiration, and of the organs of locomotion. Synchronous with this, or somewhat later, there is found hyperæsthesia of the nervous centres, with tonic, cataleptic, and tetanic cramps, and sometimes anæsthesia and paralysis.

In one case, the injection of five centigrammes into the veins of a cat brought on death in a few minutes. A smaller dose produced death in a few hours. In addition to the tonic and clonic spasms, there was observed salivation, liquid stools, disturbed respiration and circulation, dilatation of pupils, reduction of temperature, and anæsthesia. A like dose, introduced under the skin, excited salivation and vomiting, then

adynamia, very laboured respiration, reduction of temperature, with a tendency to fright and spasmodic and paralytic phenomena.

Large dogs were not destroyed when 5 centigrammes were given by the stomach. But a dog who had survived such a dose succumbed in two minutes after the injection of a like quantity into the jugular; while another, larger and older, was not destroyed by the injection of 25 decigrammes in *the crural vein*. (This difference of result is remarkable; was it on account of the size and the race of the animal, or the vein into which the injection was made? It is unfortunate that this experiment was not repeated.) Whatever the modes of administration, dogs were purged, and food in the stomach produced vomiting. Rabbits died in an hour or an hour and a half, with 3 decigrammes to 5 centigrammes, presenting symptoms analogous to those exhibited by the dogs.

Necroscopic examination exhibited no alteration sufficient to explain the death. There was only found an inequality in the distribution of the blood, only hyperæmia of some and anæmia of other organs; the heart, liver, and larger vessels contained much black blood, possessing all the characteristics of venous blood. All the other alterations were insignificant.

The pathological disturbances caused by caffein are of different kinds; but the most important occur in the nervous system. It destroys by exhaustion of nervous power, and seems to act especially upon the heart and the parietes of vessels.

Tobacco as an Antidote for Strychnia. By Dr. THOMAS O'REILLY.
(*'Dublin Med. Press,'* June 23, 1858.)

CASE.—On Thursday, September 10, 1857, at one o'clock p.m., I was urgently requested by Dr. Byrne to accompany him to see a Mr. Johnson in this city (St. Louis, Missouri, U.S.), who, he was informed, had taken poison. On arrival at his residence we learned the following history to account for his condition:—After a three years' cruise, as a musician on board an United States frigate, he was paid off in New York, and for the amount purchased drafts on St. Louis, which on his arrival here were found to be worthless. This, together with recent domestic sorrows, so overwhelmed him, that he determined on self-destruction. To accomplish this end, he called on a respectable druggist, and demanded a large dose of poison for a dog. The druggist gave him six grains of strychnine, which he carried into an adjoining beer-house, and, playfully remarking to the bar-tender that he was going on his last spree, mixed the strychnine with beer and drank it off. Soon repenting this rash act, he mentioned to those present that he had taken poison, and wished they would give him an emetic. One was procured which vomited him freely, but, notwithstanding, violent symptoms set in, and we were called on to see him.

On entering his room we found him stretched on his back, his countenance expressive of the most painful suffering and distress, his frame shaken by frequent convulsive spasms, his limbs rigidly extended, and his head slightly bent backwards. His face was of a livid red, and covered with a profuse sweat; his eyes were sunken, and moved with a rapid motion; his mouth was covered with saliva, which he ejected by spasmodic jerks, strongly re-

minding me of a case of hydrophobia which I had seen some time before. His respiration was quick and difficult, and attended with great pain in the precordial region; skin warm, and covered with a profuse clammy sweat, and he had copious watery discharges from his bowels. His intellect clear and collected, and his feelings were so morbidly acute that the slightest touch appeared to aggravate his sufferings and bring on a spasm. On this account we could not correctly ascertain the state of his pulse.

Ignorant of any antidote likely to relieve him, and pressed by the urgency of the case, Dr. Byrne, acting on the suggestion of Dr. Haughton's paper (read before a meeting of the Royal Irish Academy, November 29th, 1856), took a cigar from a gentleman present, and infused it in half a pint of water.

One hour and fifteen minutes after he had taken the poison we gave him the first dose of the tobacco infusion, which he swallowed with difficulty. We continued it in tablespoonful doses at intervals of five minutes, until he had taken half the quantity infused, before we had noticed a favorable change. Then the muscles became relaxed, the spasms less severe, and the intervals between them longer; and so conscious was the patient of relief, that he constantly called for the tobacco juice when he felt the paroxysm approaching. This encouraged us to persevere with the infusion, prolonging the intervals between each dose, as the frequency of the spasms abated; until finally, after twelve hours, they disappeared, leaving him in a state of fearful nervous prostration, from which he recovered in a few days, under a careful tonic treatment.

In the quantity of infusion administered we used one ounce and two drachms of dry tobacco leaves, including the cigar—a quantity which no healthy condition of system could stand; but the urgency of this case demanded it, and the result justified it, and at the same time afforded us another illustration of the counteracting influence of poisons, and tended in some way to establish the correctness of the conclusions at which the Rev. Mr. Haughton had arrived by his experiments.

On the Action of certain Vegetable Diuretics. By Dr. WILLIAM A. HAMMOND, Assistant-Surgeon, U. S. Army. ('American Journal of Med. Sciences,' January, 1859.)

The ensuing investigations consist mainly of repetitions of those performed some years since by Krahmer, and subsequently by Bird. They have reference to the appreciation of the influence of squill, juniper, digitalis, and colchicum, over the quantity of the urine, its specific gravity, and the amount of its solid organic and inorganic constituents. They were all performed upon healthy adult males.

The quantity of urine was determined in cubic centimetres, and the weight of solids in grammes.

The method employed for the determination of the whole amount of solid matter was as follows:

Ten cubic centimetres of the urine were evaporated to as complete dryness as possible *in vacuo* over sulphuric acid, and the residue accurately weighed. By simple proportion the amount of solids in the whole quantity of urine was easily ascertained.

Although it is impossible to get rid of all the water by this process,

the quantity remaining is extremely small, and the results obtained are far more accurate than those yielded by evaporating to dryness in the water-bath as generally practised. No matter how carefully this latter method is conducted, the loss of urea by decomposition is always an important item, and involves far more serious errors than the imperfect desiccation by the former process.

For the determination of the amounts of organic and inorganic constituents separately, the solid residue obtained as above was mixed with ten or fifteen drops of moderately strong nitric acid, and gently heated till the mass was well dried. The heat was then gradually raised till all the carbon was consumed, and the mass in consequence became white. It was then cooled *in vacuo* over sulphuric acid and weighed. The inorganic matter was thus determined and the loss showed the proportion of organic substance.

Digitalis.—The subject of the experiments with this substance was about twenty-five years of age, and in good health. For the three days immediately preceding the commencement of the investigations the average quantity of urine daily excreted by him was 1474·5 cubic centimetres, the specific gravity was 1024·30, and the average total amount of solid matter was 75·31 grammes, of which 30·17 grammes were inorganic, and 45·14 organic constituents. The digitalis was given in the form of the officinal tincture in doses of 20 minims three times in 24 hours, and was continued for three consecutive days. During this period the manner of living (food, drink, exercise, &c.,) was as nearly as possible the same as during the preliminary investigations.

1st day. The urine passed on this day was of a pale straw colour and feeble acid reaction; quantity 1950 cubic centimetres; specific gravity 1013·25; total solids 69·98 grammes, of which amount 31·27 were inorganic and 38·71 organic matter. The action of the digitalis was not manifested otherwise than by its effect upon the urine.

2d day. The urine passed on this day was of similar physical character to that above mentioned. The quantity was 1873·6 centimetres, the specific gravity 1014·32 and the total solids 63·74 grammes. The inorganic solids amounted to 30·15 grammes, and the organic to 33·49.

The pulse on this day was somewhat slower and fuller than on the previous day.

3d day. The quantity of urine evacuated on this day was 1624·9 cubic centimetres, and of specific gravity 1020·04. The total amount of solid matter was 67·29 grammes, of which 33·19 were inorganic and 34·10 organic.

The colour, reaction, and odour of the urine were similar to those of the two previous days.

The characteristic effects of the digitalis upon the action of the heart were well marked during this day.

The effect of the digitalis in increasing the amount of urine is seen to have been greatest on the first day. On the second day it had fallen somewhat, and on the third was but 150 cubic centimetres greater than when no digitalis was taken. The solids, it is seen, were less than the normal standard from the commencement, were still further reduced on the second day, and on the third were slightly increased. This diminution is perceived to have been owing to the lessened amount of organic

matter excreted. The inorganic substances were somewhat increased in amount over the ordinary proportion.

Juniper.—The experiments with this substance were conducted on a healthy man thirty-five years of age. The average condition of his urine for the three days immediately preceding the investigations was as follows: quantity 1237·5 cubic centimetres, specific gravity 1022·50; total solids 61·23 grammes, of which 23·12 were inorganic, and 38·11 organic matter. It was of ordinary colour and odour, and of strong acid reaction.

Sixteen ounces of the officinal infusion of the berries of *Juniperus communis* were taken during the twenty-four hours, and the manner of living kept as nearly as possible to correspond with that of the preliminary experiments.

1st day. For this day the quantity of urine amounted to 1732 cubic centimetres, the specific gravity of which was 1016·38; the total solids were 62·75 grammes; of this amount 25·43 grammes were inorganic, and 37·32 organic constituents.

The urine was of a pale straw colour, and gave off the characteristic odour produced by juniper. The reaction was feebly acid.

2d day. The quantity of urine passed on this day was 1885·2 cubic centimetres. The specific gravity was 1014·15, and the total solids 58·49 grammes, 22·17 of which were inorganic, and 36·22 organic matter. The physical characteristics were similar to those of the day before. The reaction was barely acid.

3d day. On this day the quantity of urine was 1672·5 cubic centimetres, with a specific gravity of 1018·41. The total solids amounted to 63·27 grammes, of which 27·50 were inorganic, and 35·73 organic matter. The physical characteristics and reaction were the same as on the previous day.

From these experiments it is seen that whilst the quantity of urine was materially increased by the juniper, the amount of solid matter, as a whole, was but slightly affected, the loss in organic matter being about compensated for by the increase in the inorganic.

Squill.—The experiments with this substance were instituted upon the author, and conducted upon the same general principles as the foregoing series. The average daily quantity of urine, for the three days preceding the investigations, was 1358 cubic centimetres. The specific gravity was 1023·51, and the total solids 69·35 grammes; of this amount 27·22 were inorganic, and 42·13 organic matter.

Two grains of the dried bulb of the *Scilla maritima* were taken three times in the twenty-four hours. The other conditions remaining the same as in the preliminary examination of the urine.

1st day. The quantity of urine passed on this day was 1572 cubic centimetres of 1020·34 specific gravity. The total solid matter was 6067 grammes, 31·07 of this amount being inorganic, and 29·60 organic constituents. The urine was of feeble acid reaction.

2d day. Quantity of urine 1493·5 cubic centimetres, specific gravity 1020·90, total solids 58·22 grammes, inorganic matter 30·15, organic 28·07 grammes. The reaction, &c., were the same as on the preceding day.

3d day. On this day the quantity of urine amounted to 1535 cubic centimetres, and was of 1019·37 specific gravity. The total amount of

solid matter was 61·58 grammes, of which 30·53 were inorganic, and 31·00 organic constituents. The reaction, colour, &c., were unchanged.

From the above experiments it is perceived that the action of the squill was similar to that of the digitalis and juniper, *i. e.*, causing an increase in the water of the urine and inorganic solids, but a reduction of the amount of organic matter. The loss of organic matter was considerably greater than with either of the other substances.

Colchicum.—The investigations into the action of this substance were performed upon a healthy man 28 years of age. The urine for the three days immediately preceding the commencement of the experiments, was of the following daily average character: Quantity 1230 cubic centimetres, specific gravity 1025·08; total solids 63·12 grammes, inorganic matter 29·83, and organic 33·29. The reaction was very strongly acid.

One and a half drachms of the officinal tincture of the seeds of the *Colchicum autumnale* were given three times in twenty-four hours, and continued for three days. During this period the food, exercise, &c., were as nearly as possible the same as during the preliminary series.

1st day. The quantity of urine passed on this day was 1595·7 cubic centimetres, with a specific gravity of 1024·37. The total solids amounted to 77·29 grammes, the inorganic matter of which was 36·50 grammes, and the organic 20·79 grammes. The reaction was strongly acid.

2d day. Quantity of urine 1484·1 cubic centimetres, specific gravity 1024·31; total solids 75·22 grammes. The reaction was very strongly acid.

3d day. On this day the quantity of urine amounted to 1620 cubic centimetres, and was of 1022·06 specific gravity. The total amount of solid matter was 79·33 grammes, of which 34·20 were inorganic, and 45·13 organic constituents. Reaction strongly acid.

It is thus perceived that the action of the colchicum, as compared with that of the other substances experimented with, was very remarkable, it being the only one with which there was an increase in the amount of solid matter eliminated, both organic and inorganic.

From the foregoing experiments the following table embracing the averages of each series of investigations is constructed.

	Quantity of urine.	Specific gravity.	Total solids.	Inorganic solids.	Organic solids.
Normal standard . . .	1474·5	1024·30	75·31	30·17	45·14
Digitalis	1822·8	1015·87	67·00	31·54	35·43
Normal standard . . .	1237·5	1022·50	61·23	23·12	38·11
Juniper	1763·2	1016·28	61·50	25·03	36·42
Normal standard . . .	1358·0	1023·51	69·35	27·22	42·13
Squill	1533·5	1020·20	60·15	30·60	29·55
Normal standard . . .	1280·8	1025·08	63·12	29·83	33·29
Colchicum	1556·6	1023·58	77·28	35·23	42·04

From the foregoing investigations, it is deducible that neither digitalis, juniper, nor squill, increases the total amount of solid matter eliminated by the kidneys, and that the organic matter is considerably reduced through their influence. Although they do increase the amount of inorganic matter removed through the urine, yet as it is the organic matter which is generally considered as contaminating the blood in disease, it is evident that they exert no effect whatever in depurating this fluid, but on the contrary are positively injurious.

The results obtained, in so far as the experiments with digitalis, squill, or juniper are concerned, are similar to those obtained by Krahmer, but are materially different as regards the colchicum. For, although Krahmer found that under the influence of this medicine there was an increase in the amount of organic matter excreted, this was so small as to lead to the supposition that it may have been accidental, and besides there was a reduction in the quantity of inorganic substance removed. It is desirable, therefore, that we should have further observations with this article.

On the Therapeutic Action of the constant Galvanic Current. By Dr. ROBERT REMAK. ('Edinburgh Monthly Journal,' August, 1858.)

"After the discovery of the voltaic pile," says Dr. Robert Remak, "many experiments were made with a view to the application of the galvanic current, as a means of treatment in diseases of the nerves and muscles. Stimulated by the admonitions of Alexander Humboldt, Loder of Jena and Grassengiesser of Berlin were amongst the first to treat various paralytic affections of the limbs and sensorial nerves in this manner. The opinion at that time being, that not the continued action of the current, but the shocks produced by the interruption of the current, are the best means of exciting the normal action of the nerves and muscles, these trials could not result in success; because, as my researches have now proved, such shocks can in very few cases be advantageously employed. Thus, for many years, the electrical machine formed the only means of producing and applying electricity to the above-named diseased conditions, and the opinion was generally adopted by physicians that it signified little from what source electricity for medical purposes was produced.

"Although the discoveries of Becquerel and Wollaston (1820-30) supplied the means of producing a constant and equable galvanic or electric current, physicians did not avail themselves of these improvements, but preferred using the magneto-electrical and electro-magnetic machines which were constructed after the discovery of the current of induction, made about the same time (as Becquerel and Wollaston's) by Oersted and Faraday. In experimenting with these instruments, the fact was overlooked that the current of induction cannot be applied to the living body without producing shocks (*i.e.*, spasmodic contractions of the muscles), and that these shocks have in many cases a weakening effect. At last, the chief consideration came to be, the *easiest* method of procuring electricity for medical purposes. So, at last, the magneto-

electric machine (of Saxton Stöhrer, &c.) gave place to the self-acting electro-magnetic (galvano-magnetic) machine of Faraday, which was recommended by Duchenne of Paris, and was much approved of in France and Germany. In the work of that physician, it was stated that muscles are made to contract most readily by the current of induction, if certain points on their surface are touched by the electrodes. Having been occupied for many years with physiological researches upon muscular contraction, I was not a little curious to know the nature of these mystical points, and on directing my attention to this subject, I soon found that they corresponded with the points of entrance of the muscular nerves; and that the degree of contraction of a muscle was proportionate exactly to the number of motory nerve fibres embraced by the current at its point of application.

"I stated the result of these physiological investigations in a small pamphlet, published in 1855, and drew attention to their value in a practical point of view. It seemed to me quite obvious that, for the successful therapeutical application of electricity, a better and more extensive physiological basis was required than was afforded by this one fact; so, in continuing my researches on the subject, I was led to examine the effect of the *constant* galvanic current (as it is produced by the elements of Daniell, Grove, and Bunsen) on the muscles and nerves of a healthy man. The results of these further observations I noticed in an appendix to the second edition of the pamphlet last quoted, also in a note sent to the Academy of Paris. After having continued such researches, made upon my own person and upon many healthy men, for about six months, I was induced, in July, 1856, to apply the constant current as a means of treatment of contractions of muscles in cases of hemiplegia from cerebral apoplexy. The most important result of this application was the fact that the continued current, applied for a few minutes to a contracted muscle, had the effect of immediately *relaxing it to a certain extent, and rendering it amenable to the influence of volition.*

"After repeating these trials upon 200 cases of different kinds of nervous diseases, I reported, as the result of my further experience, that the continued application of the *constant* current to the nerves and muscles in rheumatism, paralysis, atrophy, neuralgic and spasmodic diseases, had, in my hands, proved of much greater service, in a curative point of view, than any other application of electricity had hitherto done. I gave my results in a memoir which I read before the Academy of Sciences at Paris, in séance 22d September, 1856; and, on the 29th of the same month, I made some experiments before M. Rayer, who was representative of a commission appointed by the Academy for the purpose; and, on the following day, repeated them before a number of physicians and naturalists.

"From that date to the present I have spent all my time in continuing these researches, and in endeavouring to develop the methods of application of the constant current. In the Society of Medical Science of Berlin, I stated (January 19th, 1857) that the diseases in which the constant galvanic current is of service are—

- "1. Rheumatism, acute and chronic,
2. Cerebral Hemiplegia,
3. Paraplegia,
4. Atrophy of Muscles,
5. Chorea,
6. Stammering,
7. Trembling of Limbs,
8. Cramps of Authors, &c,

and I have supported my assertions by presenting cases of these descriptions, which were either cured or improved by this mode of treatment. My subsequent publications on the subject were directed principally to the treatment of atrophy and *paralytic* diseases. This was accounted for by the fact that certain physiological views, dating from the time of Volta, Marianini, and Nobili, gave rise to the opinion that the continued action of the current had only a relaxing or weakening effect, and could do good only in spasmodic diseases. I have refuted this idea by showing that the continued current, applied in a certain manner, is of use in many cases of paralysis, in which the interrupted current of induction is even injurious.

"One of the most curious results of my researches made during the last year, is the observation that the constant galvanic current, by dilating the blood-vessels and by promoting absorption of exudations, can be used with good effect in certain inflammations, in which the other antiphlogistic and resorbent remedies are insufficient. There is no doubt that the electrolytical power of the constant current (discovered by Nicholson and Carlisle, 1800) is a powerful means of producing these effects. I have already seen most convincing and gratifying proofs of this fact, not only in rheumatic and traumatic inflammations of joints, but also in the inflammatory states of the spinal cord, which sometimes precede atrophy of this organ.

"It may readily be supposed that the mode of applying the current must be very different, in order to produce the various actions I have hitherto distinguished: viz., 1st, the catalytical (*i.e.*, electrolytic, antiphlogistic); 2d, the antiparalytic; 3d, the antispasmodic. But, for information on this subject, I am obliged for the present to refer to my various publications.

"In Germany, my experiments have not been repeated; but in France, after my visit in September, 1856, some physicians made some trials upon the subject, and, as I am now informed, these were not unattended with success. In Britain, I do not know if anything has been done in this way; but it is the object of this short paper to direct the attention of my British colleagues to this discovery (as I dare to name the methodic application of continued galvanic currents to diseased conditions), and I would feel satisfied if I thought that the methods described by me, in a work I am about to publish, would in any way assist them to the successful practice of their art.

"Finally, I think it my duty to state *distinctly*, that all my experiments, upon 700 patients, have been made only with the *constant* galvanic current (of Daniell, Grove, and Bunsen), and that there is reason to doubt if these results could have been obtained by the use of other elements less constant."

Formula for a new Elixir of Pepsine. By M. MIAHLE, (Bull. Gén. de Thérap., June 30, 1858.)

M. Miahle makes the pepsin into a generous wine, with the addition of some alcohol, and adds sufficient sugar to hide the nausious taste of the ferment. This preparation, he tells us, will keep much longer than the preparation recommended by M. Corvisart, (pepsine and syrup of cherries,) and it is so agreeable to the taste, that children will take it readily. The formula is

Amylaceous pepsine (Corvisart and Boudault) ℥iss, distilled water ℥viss, white wine (vin de Lunel,) ℥xiiss, white sugar, ℥viij., spirits of wine (*S.G.* 852°) ℥ij. These are allowed to stand until the sugar is dissolved, and then carefully filtered. One tablespoonful, which is equivalent to 15 grains, is to be taken immediately after each meal.

On the employment of Carbonic Acid as a Medicinal Agent. By M. FORDOS. ('Dublin Hospital Gazette,' Sep. 16, 1858.)

At the end of the last century some experiments were made by medical men in England on the effects produced by gases when used as medicinal agents. A very curious experiment of Ingen-Housz, a Dutch physician, served as the starting point for these investigations. Ingen-Housz found that a finger, from which the cuticle was removed, and which was painful when exposed to the air, was still more so in oxygen gas, but that the pain ceased when the finger was placed in carbonic acid gas. It appears, however, that this fact had been previously known in France. Beddoes, in England, repeated the experiment of Ingen-Housz and confirmed what the latter had stated. He directed attention to the local application of carbonic acid in the treatment of ulcerated surfaces.

In 1794, Ewart treated two cases of cancer in the breast with carbonic acid, and obtained good effects. Under the influence of this treatment the pain disappeared; one of the patients was soon cured, and in the other a sensible improvement took place. Notwithstanding these interesting results, it does not appear that the local application of carbonic acid has been submitted to further investigation since that time, probably on account of the difficulties experienced in the application and management of the gas.

In 1834, M. Mojon, a professor of Genoa, employed carbonic acid gas with success in the treatment of amenorrhœa, and the severe pains which, in dysmenorrhœa precede or accompany the menstrual discharge. He considered carbonic acid to be a powerful depressant, and an excellent antiphlogistic.

About the same time the use of carbonic acid, as furnished by mineral waters, began to be advocated. It was thus used at St. Alban, in France, in cases of rheumatism.

In 1830, Dr. Heidler organized the carbonic acid baths at Marienbad; and a little later, in 1840, the same was done at Nauheim.

In fact, for several years past, carbonic acid gas has been used at most

of the baths in Germany and elsewhere. The gas is collected from its sources and conducted through tubes into chambers, where it is used in different ways. It is used in baths for rheumatism, sciatica, and certain cases of paralysis. It is also applied locally to the eyes and the ears. It appears to act as a stimulant to the skin and to the circulation.

In 1855, Simpson of Edinburgh, recommended carbonic acid for the treatment of painful affections of the uterus and its vessels, such as uterin cancer, utero-vaginal neuralgia, dysury, vesical irritability, &c., and he cites numerous instances of cure.

In 1856, M. Scanzoni employed injections of carbonic acid to induce artificial *accouchement*.

At the same time M. Follin, a surgeon in the Paris hospitals, made experiments with carbonic acid gas, and published an interesting paper on its use, in which he enters largely into the history of the subject.

Subsequently, Messrs. Demarquay, Maissonneuve, Jobert de Lamballe, Broca, Gosselin, &c., have employed the *douche* and *injections* of carbonic acid in various affections, and have obtained excellent results.

Lastly, M. Ch. Bernard, physician to the hospitals of Paris, has published a series of observations on the good effects obtained from injections of carbonic acid in cancerous affections of the uterus.

All the experiments made with carbonic acid prove that this gas may be considered a powerful anæsthetic, which may be used with advantage in many cases; they also prove that it is possessed of medicinal properties, for the effects produced are not confined to the production of insensibility, but amelioration or cure of disease sometimes occur under the influence of this treatment.

The following simple form of apparatus has been constructed with the view of facilitating the application of carbonic acid in the form of injection or *douche*, and also for applying this gas or hydrogen mixed with the vapours of chloroform, ether, amylene, creosote, or other anæsthetic agents.

Description of Apparatus.—The flask is of thick glass, and may have a capacity of about two pints. A tin tube is made to fit into the mouth of the flask. In the interior of this tube there are some fragments of marble, and above this some pieces of sponge, the object of which will be explained hereafter. The bottom of the tube is pierced with holes to allow the passage of the gas from the flask, and the top is closed with a cap, which screws on, or may be removed at pleasure. A short metallic tube, to which a flexible tube is attached, passes off laterally from near the cap, and this is used for directing the gas to the diseased parts.

Application of Carbonic Acid Gas.—When the apparatus is required to be used for the application of the gas, the tin tube is removed from the mouth of the flask, and 460 grains of tartaric acid in large crystals are introduced, so as to lie at the bottom of the vessel; over this are placed 580 grains of powdered bicarbonate of soda, and half a pint of water. The reaction and disengagement of gas will continue for about fifteen or twenty minutes without agitating the flask; afterwards agitation is to be resorted to from time to time when the action ceases. The two reacting agents being used in the solid state, the gas is disengaged gradually as these are dissolved by the water. The carbonic acid, before entering the flexible tube, passes through the space occupied by the

pieces of marble and the sponge, and is thus deprived of any particles of saline matter which may be mechanically suspended in it.

Dr. Fordos recommended the placing of the crystals of tartaric acid at the bottom of the vessel, under the bicarbonate of soda, as the carbonic acid being disengaged from the surface of the crystals, keeps the carbonate of soda in a state of agitation, and thus promotes its solution. A regular and abundant disengagement of gas is thus ensured. If the bicarbonate of soda be at the bottom of the vessel beneath the acid, the spontaneous disengagement of gas becomes suspended much sooner, and more frequent agitation of the vessel is rendered necessary.

Application of Carbonic Acid charged with the vapour of Chloroform or other volatile liquid.—The sponge occupying the upper part of the tin tube is intended to be used in cases such as are here contemplated.

Hardy of Dublin has employed with success the vapour of chloroform for producing local insensibility, and for applying it he has contrived a very ingenious apparatus.

The experiments of Hardy have been repeated by Dubois, Figuier, Aran, Jules Roux, &c. In Hardy's apparatus the chloroform is conveyed in a current of air, the presence of which must be objectionable. Dr. Fordos thinks it preferable to substitute carbonic acid for air, and that the simultaneous use of two substances possessing anæsthetic properties, chloroform and carbonic acid, would produce insensibility more promptly and surely. The experiments that have been made in this way have proved highly satisfactory.

In order to charge the carbonic acid with the vapour of chloroform, a drachm or a drachm and a half of chloroform is to be poured over the sponge contained in the tin tube.

Application of Hydrogen charged with the vapour of Chloroform.—The same apparatus may be used for applying hydrogen gas charged with anæsthetic or medicinal vapours. It is only necessary in this case to substitute for the tartaric acid and bicarbonate of soda some pieces of zinc and dilute sulphuric acid.

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